SOME NEW

Experiments,

WITH

OBSERVATIONS UPON HEAT,

CLEARLY SHEWING

THE

Erroneous Principles

OF THE

FRENCH THEORY.

ALSO,

A Letter to Henry Cavendiſh, Eſq.

CONTAINING SOME

Pointed Animadverſions; with Strictures upon ſome late

CHEMICAL PAPERS

IN THE

PHILOSOPHICAL TRANSACTIONS,

AND

OTHER REMARKS.

2/224

BY ROBERT HARRINGTON, M. D.

> " *Magna eſt veritas et prævalebit.*——CICERO."
> " ———nothing extenuate
> " Nor ſet down aught in malice.——SHAKESPEARE."

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SOME NEW

EXPERIMENTS,

WITH

OBSERVATIONS UPON HEAT.

THE repulſion of fire is one of the moſt intereſting ſubjects that chemiſtry can inveſtigate; and it is a principle which has never been introduced in explaining the phenomena. Fire and matter have a very ſtrong attraction for each other, and this attraction, when it takes place, that is, when fire and matter are united together, can only be decompounded in two ways. Firſt, by other bodies having a greater attraction for either of the compound bodies than they have for each other. As in lime; thus the calcareous earth has a ſtrong attraction for fire, producing lime; but, if an alkali is added to the lime there is a decompoſition; the alkali having a ſtronger attraction for the fire, attracts it from the calcareous earth; and the fixed air of the alkali is attracted by the calcareous earth.

Alſo, the lime may be decompounded by the joint operation of fixed air and water, but neither can do it ſeparately.

But, Secondly, the moſt common way by which fire is ſet looſe from its chemical combination, is by combuſtion; which operates principally by repulſion. Thus, when a body is ſet on fire, the pure air's fire is ſet looſe, and alſo the combuſtible body's fire, which produces ſo ſtrong a heat, that the fixed fire, which both the air and the burning body poſſeſſes, is repelled from its chemical combination, ſo as to become free, or actual fire. A ſpark being applied to the combuſtible body, ſets free the fire of the air contiguous to it, then both fires act by repulſion, upon the combuſtible body's fire, and ſo on; as one part of the air and burning body's fire is ſet free it acts upon its neighbouring part till the whole body is conſumed. Metals attract their fire very ſtrongly, ſo that they require a very ſtrong application of free fire to be decompounded: gold will not burn; (without the aid of nitre) it comes out of the fire pure. But many of the other metals will burn, iron, for inſtance; but this decompoſition of the metal, or the earth of iron, parting with its fire, is greatly aſſiſted by the earth having a great attraction for the water, and fixed air, which pure air is formed of; ſo that, in the combuſtion, they will both enter the earth of iron. This is a general principle in chemiſtry, that, as bodies part with one body, they greedily attract others; metals have different degrees of attraction for fire, the noble metals have the ſtrongeſt.—See Gent. Mag. for 1792, part 2, p 619.§

Therefore,

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§ 'In addition to the proofs which I have given in my different publications, that the body which Stahl, Scheele, Dr. Prieſtley, &c. call phlogiſton, is fixed fire, and not an element *ſui generis*, I ſhall obſerve,

'That, by expoſing iron to the nitrous acid, an active fermentation, and a great generation of nitrous air, is produced, and the iron is reduced to a calx. If iron and water are expoſed to atmoſpheric air, the iron will be reduced to a calx, the pure part of the air will be imbibed by the calx, and an oily ſcum will ſwim upon the water. This was the reſult of Mr. Scheele's experiment; who ſays, "pure water only can produce inflammable air from iron; it is a ſcum which conſtantly appears on the ſurface of the water after it had ſtood over filings for ſome weeks, and has been ſomewhat ſtirred." If this oil is carefully ſeparated from the water, it will, with the nitrous acid, form nitrous air; or, if applied to the calx of the iron, the iron will be reduced, and, in its reduction will part with the air it had imbibed from the atmoſphere, not in the ſtate of pure, but of fixed air. This I have already fully explained in my former publications.—Hence it appears that it is an oily body which forms the nitrous air, and the earth of the metal into its metallic ſplendour. If heat is applied to this oily body, it will form inflammable air, and, if burned in the ſtate of oil, will turn pure air into fixed air: but (as I have elſewhere fully proved) in the ſtate of inflammable air, it will turn it into an acid and water.

'And, to corroborate this doctrine, if I take common oil, it will produce the ſame phenomena, i. e. it will reduce the calx, and with the nitrous acid will generate nitrous air; or, if I burn it with pure air, it will turn into fixed

Therefore, when fire alone is applied to the decompoſition of combuſtible bodies; the moſt intenſe degree of it is required, and the moſt intenſe degree of fire that the art of man can apply, is the electrical fire. See Mr. Cuthbertſon's experiments.

“ Mr.

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fixed air; or, if I form it into an air by heat, as Dr. Prieſtley has done, it will explode with pure air, leaving an acid and water in the reſiduum. Atmoſpherical air, when ignited, loſes the fire which neutralized its fixed air and water, which are conſequently precipitated. This is ſtrikingly ſeen in paſſing the electric ſpark through it; it is then formed into fixed air and water. And this is the caſe when iron is burned, and it is abſorbed by the iron.

‘ If filings of iron are dropped into water impregnated with fixed air, the water and fixed air will attract the iron, the acid air and part of the water will be imbibed by the calx, and the phlogiſton of the metal will be ſeparated. But, agreeably to the opinion of ſome chemiſts, the fixed air ſhould not be imbibed altogether, but only be decompounded from the pure air, of which they ſuppoſe it to be formed, and ſhould enter into the calx, and the carbone be left in the water; which is not the caſe. And I am aware that many will ſay, the iron is calcined by the decompoſition of the water, and not of the fixed air, in this proceſs. But as inflammable air is generated here the ſame as when vitriolic acid and water are uſed, we are right in ſuppoſing it to be from the ſame cauſe.—And not a doubt can remain but that it is from the fixed air entering the calx, as it diſappears in the proceſs. Then certainly it is the vitriolic acid that enters the calx in the vitriolic ſolution And, that not a doubt may remain but that the operation in both caſes is the ſame, let water be impregnated with fixed air, then acidulate an equal quantity of water to the ſame proportion of acid with vitriolic acid, the ſmalleſt quantity of which is ſufficient; then

"Mr. Cuthbertſon has ſince found that, with a machine with two plates of eighteen inches diameter, he could charge a ſingle jar of 160 inches of coated glaſs ſo highly as to make an inch of the ſmalleſt iron wire vaniſh into ſmoke, and three inches exhibit the phenomena of philaments floating in the ſmoke into which the wire was converted."

And

let iron be added to each of theſe portions of water, thus acidulated, and in both the acid will diſappear, entering the iron, forming it into a calx, and what has been called the phlogiſton of the iron will be ſeparated. And to adduce a ſtill more unexceptionable proof; if the ſolution of iron, which is formed from the fixed air, be precipitated by the cauſtic alkali, or by other means, and an acid be added to the precipitate, fixed air will be expelled from it, pure and unchanged.

But, as Stahl and the philoſophers after him, ſeeing phlogiſton paſs from one body to another, and having no idea of the chemical attraction of fire, ſuppoſed it to be a body different from fire. I ſhall ſhew, in oppoſition to that idea, that *actual* fire, or perhaps, more properly ſpeaking, fire in its free diſengaged ſtate, when ſenſible to our feelings, and to the expanſion of mercury in the thermometer, is equally under the influence of chemical attraction, paſſing from one body to another; and that, during its tranſitions, being under the influence of chemical attraction, it does not diffuſe itſelf, but paſſes into the attracting body the ſame as an alkali into an acid.

'I would deſire the reader to pay particular attention to what may be brought in proof of this, becauſe it has never been properly underſtood or attended to.

'Lime and cauſtic alkaline ſalts have loſt their fixed air, or aerial acid and water, and in their ſtead have united to fire. If lime, for inſtance, is mixed with the fixed alkaline

And this electrical fire, from its intenſity, can decompound pure air, which is nature's great agent for combuſtion; for, Dr. Prieſtley, Mr. Bergman, &c. burnt pure air into fixed air, by the electrical fire.

Alſo,

alkaline ſalt, the alkali having a ſtronger attraction for the fire of the lime than for its own fixed air, will attract the fire of the lime, which has a leſs attraction for it than the alkali has; conſequently, the fire will leave the lime, and be attracted by the alkali.* This cannot be from the lime having a ſtronger attraction for the fixed air, or aerial acid, than the alkali, as alkalies attract acids more ſtrongly than calcareous earths do.

'In conſequence of this miſtake, Dr. Black is wrong and contradictory in his table of attractions. He ſays,

Acids.	Fixed air.
Fixed alkali,	Calcareous earth,
Calcareous earth,	Fixed alkali,
Volatile alkali,	Magneſia,
Magneſia.	Volatile alkali.

But he ought to have placed the different bodies under the head of fixed air, the ſame as under the acids, for the change of affinity is owing to the fire which the calcareous earths and the magneſia, in the ſtate of lime, poſſeſſed, and which Dr. Black does not ſo much as acknowledge it does poſſeſs, not having a chemical knowledge of fire, or its affinity.

'But a ſtill ſtronger proof that alkaline ſalts have a more powerful attraction for fire than for fixed air is this: expoſe the dry cauſtic alkaline ſalt, or lime, to dry fixed air, and they will not attract it, nor part with their fixed fire; but add fixed air and water to the cauſtic ſalt, or lime, both acting together, their joint attraction will expel the fire, though neither can do it by themſelves.

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* Lime, from the fire which it poſſeſſes, will aſſiſt in reducing metals; i. e. I have found a calx will ſooner be reduced by being mixed with lime than with calcareous earths.

'It

Alſo, Mr. Cavendiſh burnt the pure air of the atmoſphere, and the pure air of the laboratory, into the nitrous acid; that azote had nothing to do in this experiment, I have fully proved in the ſequel.

Therefore,

'It is the very ſame with the vitriolic acid and iron.—The earth of iron attracts ſo ſtrongly its fixed fire, that the vitriolic acid cannot rob the earth of iron of it; but, if the acid's attraction be aſſiſted with water, the earth of iron having a ſtrong attraction for the water, their joint powers will decompound the iron, its earth attracting the water and the acid, whilſt the fixed fire is expelled as inflammable air.

'If the concentrated vitriolic acid be applied to the iron, it will not act upon it, becauſe the attraction which the earth of the iron retains for the phlogiſton, is ſtronger than that which the acid has for the earth. But, if the vitriolic acid is diluted with water, they both act together; the acid attracts the phlogiſton and the earth, whilſt the water attracts the earth.

'That the earths of metals have a very ſtrong attraction for phlogiſton may be ſtrikingly found in many inſtances. Silver ſo ſtrongly retains this attraction, that the vitriolic acid will not act upon it; but apply the nitrous acid, and it will attract its phlogiſton or concentrated fire, and fly off with it in the form of nitrous air.—Gold has ſo ſtrong an attraction for its plogiſton, that the nitrous acid will not diſſolve it. It is well known that the action of two acids, the marine and the nitrous, is required at the ſame time, or that the marine be dephlogiſticated.

'We find that metals reſiſt combuſtion in the ſame manner as they do the action of acids. Gold will not burn though expoſed to the ſtrongeſt fire; it may ſublime, but will not conſume, becauſe gold ſtrongly retains its fire. We ſhall ſhew in the *aurum fulminans* that its attraction for the concentrated fire, or phlogiſton of the volatile alkali is ſo very powerful, that, being expoſed to

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Therefore, impreſt with this theory, I have been performing ſome experiments with this electrical fire, making it as intenſe as poſſible, in order to repel the body's fixed fire,

the ſlighteſt degree of warmth, it will ruſh into the alkali, and decompound it into actual fire.*

Upon the ſame principle, the nitrous acid (as is ſhewn in my publications on this ſubject) will act upon eſſential oils; and, owing to the attraction of gold and of the nitrous acid for concentrated fire, they will ruſh into the alkali and the oils, and ſet looſe in the efferveſcence ſuch a quantity of actual fire as to produce ignition; the volatile alkali at the ſame time exploding, as it does in the combuſtible ſalt lately diſcovered by the French chemiſts, which is formed of the nitrous acid and the volatile alkali.

'It is evident that the nitrous acid and the earth of gold act in conſequence of their attraction for the fixed fire of oils and alkali, for, in the proceſs, the nitrous acid becomes what chemiſts call phlogiſticated. However, the proceſs will not ſucceed without the acid be highly dephlogiſticated; and no other calx but the earth of gold will do, which (as has been already ſhewn) has ſo great an attraction for phlogiſton, that the gold is reduced after the exploſion of the *aurum fulminans*. It was from this attraction that Mr. Scheele found the earths of gold and ſilver to be reduced by only gently throwing the rays of light on them.

'The calces of metals have a very ſtrong attraction for phlogiſton; and we are acquainted with no bodies in nature capable of greater attraction than acids, nor any that can decompound the metals but them. In reſpect to gold, it has been already obſerved, that the influence of fire, as actual, or free fire. From the analogy of nature's great agent, viz. pure air; as its fire is not only moſt eaſily ſet looſe, and by

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* Mr. Scheele found the earth of gold to have ſuch an attraction for the volatile alkali that it would actually attract it from the vitriolic acid, decompounding the vitriolic ammoniac. And the volatile alkali is known to be a phlogiſtic body, forming inflammable air with heat. See his Eſſays.

of two acids is required. Nay, it is evident from Mr. Scheele's experiments, that the calx of gold can decompound the vitriolic ammoniac, or take from it its phlogiſton. When this is done by combuſtion, it is by ſetting looſe their phlogiſton as actual fire. If gold is ſoluted in *aqua regia*, and another metal added to the ſolution, the calx of gold having a ſuperior attraction for its phlogiſton, will rob the metal of it, and will be precipitated with its metallic ſplendour. Silver is next in attraction for phlogiſton; then mercury and copper. They are well known to precipitate each other according to their different degrees of attraction for phlogiſton.

'To explain theſe phenomena, Mr. Lavoiſier ſays, it is not owing to theſe bodies having an attraction for phlogiſton, agreeably to the gradation juſt mentioned, but to their having a leſs attraction for dephlogiſticated air than the baſer metals.

'This cannot be ſo, for the calces of lead and mercury part with their dephlogiſticated air much eaſier than the other metals.

'Let us attend to a ſolution of gold in the dephlogiſticated marine acid. If the gold is precipitated by an alkali, the acid will be found in its phlogiſticatedſtate, or as common marine acid. Take a quantity of this ſolution, and add eſſential oils to it, or any other phlogiſtic bodies you pleaſe, and you will find the oil attract the calx of the gold from the acid, which will receive its phlogiſton again, and be precipitated in its metallic form. How can this be explained on the theory of Mr. Lavoiſier? The oil which is formed of his carbone ought, according to his explanation, to have united with the dephlogiſticated air of the calx of the gold, and to have formed fixed air; yet there is not an atom of fixed air formed in the proceſs. Moreover, dephlogiſticated air could not have been imbibed again by the acid, as it is left in its phlogiſtic ſtate.

by that means acts upon the combuſtible body's fire, but alſo as it poſſeſſes an acid which

aids

‘ Another phænomenon is diſcoverable from this experiment, namely, that the calx of gold has ſo ſtrong an attraction for its phlogiſton, that it will leave its acid and be united to its phlogiſton in this low temperature of heat.

‘ When the above facts are maturely conſidered, we find every thing militates againſt Mr. Lavoiſier's explanation. Beſides, it is well known that the oils and pure air will not unite but in the temperature of oils burning. Gold, from its affinity to its phlogiſton, reſiſts calcination; for, if the calx be precipitated, even the acetous acid will diſſolve it.

‘ Mr. Lavoiſier's doctrine of fixed air has nothing to ſupport it. If I drop oil, or any phlogiſtic body, into the marine acid, either in its dephlogiſticated, or phlogiſticated ſtate, they will form only marine acid air and inflammable air, not one atom of fixed air. Will any chemiſt be hardy enough to contradict the evident concluſion which follows from this experiment? It is juſt the ſame with the other acids. With theſe carbonic bodies they form only acid and inflammable airs. When metals are diſſolved in acids, they form a calx, a ſnug body in which to ſecrete the pure air of the acids; but when oils are diſſolved, they ought to form fixed air in the greateſt abundance. Hence, agreeably to this doctrine, we ſhould thus have a quicker proceſs for obtaining fixed air than by the ſolution of calcareous earths in acids.

‘ Now here we have a regular chain of phænomena clearly correſponding with this theory. But let us enquire how our modern hypotheſes agree with it.

‘ Mr. Lavoiſier, ſome time after I had endeavoured to ſet aſide the doctrine of phlogiſton being an element, from a conviction of its inſufficiency to account for the above phænomena, went to the oppoſite extreme, not even allowing any kind of fixed fire in thoſe proceſſes of metallic reduction and formation of nitrous air, aſſerting that, in the firſt, it is merely imbibing oxygen gas, and, in the other, the nitrous acid loſing its oxygen gas.

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aids the combuſtion, by uniting with the combuſtible body; it ſtruck me to ſubſtitute a body

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'The firſt thing that ſtrikes us in this doctrine is this; if you take an ounce of the concentrated vitriolic acid diluted with two ounces of water, and apply it to iron; inflammable air only is generated. If you apply the ſame quantity of the concentrated nitrous acid with an equal quantity of water, nitrous air only is generated.

'To explain theſe phenomena, it is ſaid, that the water, in the firſt proceſs, is decompounded, and, in the ſecond, the nitrous acid. Now, I ſhould think that every chemiſt muſt allow that it is the acid in both theſe proceſſes that diſſolves the metal. How then can it be conceived that, in one proceſs, the acid ſhould be the calcining body, and the water in the other? If the ſolutions are examined, the ſoluting bodies will be found to be the acids in both, for, by adding an alkali, the acid unites to it, and precipitates the earth in both the proceſſes; but water will not diſſolve iron by itſelf.

'The explanation ſeems evidently to be this (as has been elſewhere ſhewn): the nitrous acid as is well known, having a greater attraction for phlogiſton, rapidly ſeizes upon it, and from its greater volatility, flies of with it as nitrous air. On the contrary, the vitriolic acid, having a leſs attraction for it, will not decompound iron without the aid of water, for the earth of which, water has ſo ſtrong an attraction, that when the water, aided with a great degree of heat, acts upon the iron in the form of ſteam, it will decompound iron without the acid.

'That inflammable air ſhould be formed when water is added to iron, either along with the vitriolic acid, or by ſteam, appears to be from this cauſe: all airs, in entering their aerial form, have a ſtrong attraction for water, the ſame as the neutral ſalts have in entering the cryſtalline form, called, with great propriety, the water of compoſition. Of this we have a ſtriking inſtance, as I have before ſhewn, in nitrous vapour, which is perfectly condenſible, unleſs it paſſes through water,

and

a body for it, which poſſeſſed both fixed fire and an acid. Both ſulphur and phoſphorus immediately preſented themſelves as fit bodies for the experiments; I therefore mixed theſe bodies with the metals moſt eaſily, capable of being burnt, as copper, iron, &c.— Pure air alſo poſſeſſes water, which aids its agency in making bodies burn, by entering the burning body as the calx of metals; ſeen by both

and then it gets the water of compoſition for its aerial form, and becomes permanent nitrous air. Therefore, in the formation of inflammable air from iron, the water and heat ſeize upon the phlogiſton of the metal, with a ſmall proportion of the vitriolic acid, and they form inflammable air. And if a metal is reduced by fire alone, and then calcined by acids, or by water and pure air, as in the calcination of iron by theſe bodies, its phlogiſton, being ſeparated, will either take the form of an oily ſcum, or the ſtate of inflammable air. Then we muſt ſuppoſe that the fire has penetrated the metal, being cloſely attracted by it, ſo that, being ſet looſe, or parted from the calx, it will appear in its fixed ſtate united with a part of the metal in the ſtate of phlogiſton, and which, when burned, turns again into actual fire.

‘ If the vitriolic acid without water is aided by heat, it will equally decompound iron, forming it into an air ſimilar to the nitrous, viz. the vitriolic acid air.

‘ But, as aerial forms are apt to elude the ſenſes, and conſequently not to be ſo eaſily underſtood, let us take more ſubſtantial bodies, ſuch, for inſtance, as the different kinds of æthers; theſe will place the doctrine in ſo clear a light that the dulleſt capacity can hardly miſtake it.

‘ The vitriolic æther is made by adding the vitriolic acid to ſpirit of wine, by ſlow degrees, in the cooleſt atmoſphere; for, I have found that, if the acid is added too rapidly, ſo as to generate a great degree of heat, they only form a vitriolic acid air and the phlogiſticated vitriolic both fixed air and water, being required in decompounding lime, ſetting its fire free.—Therefore water would aid theſe proceſſes; but then, as our theoriſts have been endeavouring to prove that water is compounded of pure air, we muſt not make uſe of it in the experiments.

I burnt pure ſulphur and iron, without the agency of pure air, into a vitriolated iron; this I proved many years ago: and alſo, that I formed from them the vitriolic acid air, and inflammable air. But, knowing that the moſt intenſe fire is to be procured from the electrical fire, and, knowing that the intenſity is eſſential to the combuſtion, or ſeparating by repulſion, the fire which combuſtible bodies poſſeſs. I therefore expoſed ſulphur and iron to an intenſe application of electrical fire, and it anſwered my moſt ſanguine expectation. By heat thoſe bodies are eaſily diſſolved into a maſs. I got a ſmall cylinder of iron, better than an inch

olic acid†. But, by uniting them in the atmoſphere of a cold cellar, they form the vitriolic æther, the acid and the ſpirit of wine being ſo united, that they form the neutral body, æther, the acid diſappearing, being covered, ſaturated, or, more properly ſpeaking, neutralized, with the phlogiſton of the ſpirit of wine.'

† Mr. Scheele has written an admirable Eſſay on Æther, in which he clearly proves that the acids unite with the ſpirit of wine. Speaking of the vitriolic æther, p. 301, he ſays, "this, I think, will be quite ſufficient to prove that the vitriolic acid is a conſtituent part of the vitriolic æther:" And he proves juſt the ſame of the other æthers formed of the other acids and ardent ſpirit.

in length, and about half an inch in diameter; having cloſed it at one end, I filled it with this diſſolved maſs of ſulphur and iron, about one part of the former, and three of the latter. After having near filled it, I cloſed the other end of the cylinder, then drilled two holes at each end through the iron cylinder, and through a part of the maſs, ſo that I introduced two golden wires into them; bringing their points near together: the drilled holes being conſiderably larger than the wires, I poured melted glaſs into them, ſo as to inſulate all the parts of the golden wires in the maſs, except their points. Then, by drawing through the maſs the ſtrongeſt electrical fire, and alſo by examining that part of the maſs through which the fire paſſed; I found it clearly, beyond all diſpute vitriolated.—Copper anſwers, I think, better than iron for the experiment, and I generally found the internal ſide of the iron cylinder vitriolated. Sometimes I have thought that the experiment would have anſwered better if the ſulphur and the metals were placed in the cylinder without being melted, the former in powder, and the latter in filings. Sometimes, in theſe experiments, ſubſtituting phoſphorus inſtead of ſulphur.

After I heard of Mr. Cuthbertſon's experiments on metals, by electricity in the air pump, *in vacuo*, and in the different airs, I performed them in this way. The metals that were to be calcined, were placed in a ſtrong metallic caſe, with a lining of ſilk, or other electric bodies, between the metal caſe and the metals. By this means I was able to perform the exploſion repeatedly, upon the ſame metal. I have alſo repeated, in Mr. Cuthbertſon's way, my experiments on ſulphur and the metals.

I have varied theſe experiments, ſometimes making a mixture of different metals with ſulphur or phoſphorus; as equal parts of copper, mercury, lead, tin, ſulphur, or phoſphorus; and ſometimes iron, or barytes, in the place of one of the above metals.—And alſo, expoſing the above ingredients in a moſt intenſe hot crucible. But, two things ought to be attended to in this experiment, the ingredients ſhould not be added till the crucible is red hot at the bottom.—The theory of which, is this. When the ingredients unite together in a maſs, they produce ſo great a heat, that it ſets free a great part of their fixed fire: for, bodies which contain a great quantity of fixed fire, when chemically united together, a part of this fire is ſet looſe; as they require more fire to ſaturate them when ſeparated than when united. And, the quicker they are melted and united together, the better, as more heat is produced, and the greater the repulſion; and, in conſequence, the greater ſeparation of free fire. But, as both the vitriolic acid air and inflammable air are ſet looſe in theſe experiments, proved by my Letter to Dr. Prieſtley, Lavoiſier, &c. in the year 1788. Therefore, by having the pnuematic apparatus to carry off thoſe airs, it will favour the proceſs; as, when they are confined they hinder the combuſtion.

Upon hearing that Mr. Cuthbertſon had performed ſome electrical experiments upon the different metals, I was anxious that he ſhould repeat mine upon ſulphur and the metals; I therefore wrote to a correſpondent of his, the beginning of November 1797, deſiring that he would requeſt him to do it, and he immediately wrote to Mr. Cuthbertſon; I have calcined mercury, having a ſmall proportion of lead, or tin in it, both, *in vacuo*, in fixed air, or azote, by giving them the ſtrongeſt agitation in a phial, by a mill.

Dr. Prieſtley agrees with me, for we both are clear that, in burning pure and inflammable airs, there is an acid in the reſiduum; but the French chemiſts have denied it, though clearly in their experiment there was azote generated. But they conducted the proceſs by making repeated exploſions; ſo that, if there was any acid generated, it was conſtantly expoſed to the influence of fire, which might unite with the acid and be diſcharged with it.

To ſee whether fire would have any influence upon the acid, I took a tube of glaſs whoſe diameter was very ſmall; into this tube I introduced a golden wire which nearly filled it: having placed it near half way into the tube, juſt at its point I melted the glaſs around it, ſo that it was impervious to air and water; then, at the other end of it I dropt into the tube a few drops of the nitrous acid, and introduced another golden wire; and when the acid was all concentrated, in the middle, between the two gold wires, I melted the glaſs round the laſt introduced wire in the ſame manner as the firſt. Now here I had the acid placed between the two wires, and the glaſs cloſely melted near the point of the wires, ſo that there was no paſſage for the acid. Upon paſſing, for a long time together, the electric fire through the acid, I found, upon examination, it was principally diſſipated. That the paſſages were thoroughly cloſed upon the wires, and that the acid could not paſs through any opening, I was certain, by examining each end of the tube, there being not the leaſt acid. Therefore, it muſt have united with the fire and paſſed with it through the body of the glaſs; the beſt examination of the acid is to ſee what proportion of alkali it will ſaturate before and after the operation.

In Count Rumford's experiments, on the exploſion of gunpowder, we ſee how very penetrating the nitrous acid is when united to fire, fully explained in my Letter; a circumſtance never thought of by our aerial chemiſts in explaining the phenomena? and, it is clear that the exploſions of combuſtible bodies, as gunpowder, &c. their force depending upon the fire ſet looſe, are by uniting itſelf with the bodies of the compound, which gives the fire this great mechanical force.—In gunpowder the body which the fire principally unites with, is the nitrous acid: and this theory fully explains the phenomena of bodies exploding. Mr. Kirwan ſays in his Eſſay on phlogiſton, p. 31. "If ſulphur be digeſted in oil of turpentine, and then ſlowly diſtilled for ten, or twelve days, it will be converted into vitriolic acid, according to Homberg. Mem. Par. 1703. It muſt be remembered, that if this experiment be not cautiouſly conducted, it is very dangerous."

It is moſt extraordinary that the cleareſt and ſtrongeſt facts are paſſed over; while the credulous world runs to new experiments, made ſplendid to dazzle weak minds. Now how can the French theory explain this experiment? here are two combuſtible bodies gently burned, or decompounded of their fixed fire by diſtillation. Sulphur, they allow, contains no oxygen gas, nor does the oil of turpentine: from whence then came the combuſtion and the vitriolic acid. The oils contain, according to Mr. Lavoiſier, 12 oz. 5 gros. 5 grs. of charcoal, and 3 oz. 2 gros. 67 grs. of hydrogen. Here evidently thoſe two combuſtible bodies, from two circumſtances, were ignited. Firſt, the ſulphur from the vitriolic acid it contains, has a ſtrong attraction for the high concentrated fire of the eſſential oils; the attraction, or union of which, ſets part of their fire looſe; this appears clear from one of the mineral acids, viz. the nitrous, when its attraction is not blunted by being united with fire (as the vitriolic acid is in the ſtate of ſulphur) will, when united with the eſſential oils, ſo forcibly attract them, that, in their chemical union, they will produce combuſtion; that is, they will generate ſo great a temperature of heat and produce that repulſion, as to repel the eſſential oil's fixed fire from its chemical combination, ſo as to be formed into free fire. Secondly, the ſulphur and eſſential oils from the heat which they receive in their diſtillation, it will aid the combuſtion.—Therefore theſe two cauſes will ſufficiently account for their combuſtion, and the vitriolic acid being in the reſiduum.

Now, to attend to the explanation according to their theory; from whence comes all oxygen gas, not only to form the vitriolic acid, but to attract all the charcoal and inflammable air of the eſſential oils: and what comes of all the fixed air which ought to have been generated in the proceſs. Even to ſuppoſe that the oil might contain water, (but that it does not even agreeably to Mr. Lavoiſier's theory) and more particularly from the moſt concentrated oils, being the only oils which will anſwer for the experiment: but to allow that it does, what came of all the inflammable air the water is ſuppoſed to contain, as it ought to have been left in the reſiduum? But, more ſtriking than all this! from whence came all the fire ſet looſe during the diſtillation?

Can any but the moſt *blind*, *abſurd*, and *prejudiced* chemiſts reconcile this ſimple experiment to the French theory? I here, as I have done before, call upon them to explain this ſimple experiment. I call upon them in the name of *candour*, *honor*, and *juſtice*, fairly to inveſtigate and anſwer this little tract, or otherwiſe to acknowledge their errors. If the public do not inſiſt upon it they, neither do juſtice to ſcience, themſelves, or me. This publication, I flatter myſelf, will fully elucidate Count Rumford's enquiries into "heat and light." See his letter in the Phil. Trans. for the year 1797, p. 215.

By agitating mercury, and tin, or lead, in diſtilled water (See my Thoughts on Air)— And, alſo, if there is the leaſt moiſture the metals are calcined. If it proceeded from a decompoſition of the water, inflammable air ought to have been generated, which is not the caſe.

The repulſion of fire is as great and general a law in nature as gravitation. It is to this repulſion of the particles of fire to each other, that bodies are ſpecifically lighter at the equator than at the poles; for, at the ſurface of the earth it is highly ſaturated with free fire, and alſo the bodies which gravitate to it: ſo as that both their gravitating matters loſe part of their influence, being counteracted by the repulſive fire. Bodies which contain a quantity of fire in a looſe ſtate, as water, become heavier after parting with this fire, as in the ſtate of ice: for, while it contained this fire it repelled the fire which the earth poſſeſt; ſo that the earth's attraction for the gravitating particles of the water was not ſo great. Therefore, ſpeaking philoſophically, we ſhould ſay, ſuch a body gravitates to ſuch a degree; its compoſition being ſo many points of repulſion. Thus water contains an equal quantity of gravitating matter with ice, but, a greater quantity of fire; therefore it is lighter.

Oil contains a great quantity of repulſive matter, therefore it is light; but the fire it contains is more fixed than it is in water.—Fire when fixed, loſes a great deal of its repulſive quality; for, accordingly as it is chemically attracted it loſes its repulſive quality. Thus, in ſulphur, the vitriolic acid ſtrongly attracts its fire; and though it contains a great quantity, yet, from its being ſo ſtrongly combined, it is very heavy. Thus airs contain an immenſe quantity of fire, or repulſive matter; therefore, after their combuſtion, their gravitating matter ought to be heavier, as a great quantity of fire eſcapes; but then the fire eſcapes with a part of the gravitating matter, as in firing inflammable and pure airs.

Fire repels accordingly as it is concentrated together in its free ſtate. Thus, in combuſtions, when very violent, or active, what an amazing force it has. Any perſon who has ſeen an houſe on fire muſt have obſerved its amazing force and powers. In Papin's digeſter, when a great quantity of fire is attached to the water, for when let looſe what an immenſe activity and force it has? Thus, in ſteam, the greater the proportion of fire it contains, the greater its repulſion, or force, as in Mr. Betancour's experiments. But I do not believe to the ſame degree he ſpeaks of.*

That

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* The repulſion appears clear from an inveſtigation of all the phenomena wherein fire is concerned.—— Thus, in the electric fire, two electrified pith balls repel each other. But if two balls are electrified negatively, they will equally repel each other. Upon this principle the balls being electrified negatively, or not having their equal ſaturation of electricity, which, all bodies in nature poſſeſs; in conſequence the natural electricity of the atmoſphere, will be attracted to the vacuum in the balls, or

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That experiment of a bar of iron receiving a certain quantity of the ſun's rays upon it, heating it to ſuch a point: but, if all thoſe rays be collected together by a convex glaſs, or mirror, and then the iron receives them, it will be heated to a greater degree. Here the ſame quantity of rays receive a greater heating power, from having their repelling power increaſed, by being concentrated.

The theory is this, fire heats from its motion, and that motion is owing to the parti-

to its minus electricity; for this clear and ſimple reaſon. As the balls poſſeſs leſs electricity than the air, in conſequence, the air next the ſurface of the balls, its natural electricity will be leſs repelled, and, in conſequence accumulate there, or form an atmoſphere of *plus* electricity around the balls; the reſult of which is, the balls, from having an electrical atmoſphere around them, will repel each other. As the electrical fire is univerſally diſtributed through all bodies; each particle of the fire repels its neighbour, in conſequence, when a body loſes its natural ſaturation, the repulſion from that body will be taken of, and, conſequently, the neighbouring fire will accumulate around it to reſtore the equilibrium.

An electrical conductor, electrified poſitively, I have found the atmoſpere around it in different ſtates of electricity. The firſt column beyond the conductors atmoſphere, I have found negatively electrified, from the conductor repelling the air's natural ſaturation of fire.— Beyond that again, poſitive electricity, from the column of air beyond the repulſion of the conductor's electricity; the electrical fire accumulating from the column of air next to the electrical conductor, being electrified negatively, in conſequence repelling its neighbouring column leſs, producing, in conſequence, a greater accumulation, or the plus ſtate.

cles of fire repelling each other. Therefore, by being concentrated together, that motion is increaſed, from the repulſion being increaſed. Thus the ſun's rays have more heat, comparatively, than a culinary fire; from this cauſe, their motion is greater, the fire particles from a culinary fire are not ſo repelled, in conſequence, they do not give the ſame degree of light: and, moreover, the colour of bodies is owing to the power they have of reflecting, or repelling fire.— When the reflection is ſtrong they are of a white colour, red the next, and violet the leaſt. So bodies burning aſſume different colours, according to the intenſity of fire: and, in burning ſulphur, though it conſiſts of an amazing quantity of fire, yet its colour is blue; owing to this, the acid of the ſulphur has a ſtrong attraction for the fire, therefore, they partly eſcape together, and the vitriolic acid retards its repulſive powers, in conſequence its rapid motion.—Thus, in bodies burning in the pure air of the laboratory, how much ſtronger their light and heat are; or in lamps, by placing a glaſs ſo as to increaſe the current of atmoſpherical air, how it alſo increaſes their light and heat.

As we have obſerved, the colour of bodies is owing to the particles of fire, according as they are repelled: and as colour is a ſenſation, that ſenſation acts according to the force with which the retina is ſtruck; if with great force bodies appear white, if with little force violet. Juſt the ſame of ſound; ſound is conveyed to the ear by the air, as air contains a great quantity of fire: therefore, it is highly elaſtic, and, accordingly as this fixed fire is ſtruck, fixed fire is repelled, viz. that next the body which ſtrikes it; but then its fixed fire ſtrikes and repels its neighbour, and ſo till it reaches the ear or tympanum. It is from this cauſe that lightning, or gunpowder, which contains a great quantity of fire, therefore repels the fixed fire of the atmoſphere, and produces ſuch conſiderable reports as thunder and the roaring of artillery. Nature having united the fixed fire of the atmoſphere, with an acid and water, into an elaſtic aerial form; therefore, from that elaſticity, it is particularly adapted to convey ſound. Thus bodies which are the moſt ſonorus are metallic bodies, alſo containing a quantity of fixed fire; therefore, bodies ſuſceptible of repulſion.—And what ſhews the wonderful power of the Deity; the animal organs, viz. the eye and ear, are both ſuſceptible of the ſame diſtinct ſenſations, there being a direct coincidence between the priſmatic colours and the notes of muſic; the optic and auditory nerves being equally ſuſceptible of ſimilar ſenſations. But we may as well ſay that the fixed fire of the air is ſound as that the rays of light, or fire, are coloured. No, they have nothing to do with either colour, or ſound, they only give an impreſſion to the nerves, which gives that impreſſion to the brain. That nature impreſſes it with ſuch and ſuch ſenſations; and theſe ſenſations are ſo diſtinctly marked, that they do not run into one another indiſcriminately, but from ſuch an impulſe the fire receives, it will give the ſenſation of one colour, and ſuch another, &c. And the ſame of ſound, ſuch a percuſſion of the air ſhall give ſuch a note, and ſuch another, &c. and theſe different ſenſations being ſo ſimilar. How ſimple, great, and elegant is this!.

There is great difference between light and ſound; the former is from the pure fire, or rays of light ſtriking the optic nerve; the latter from fire united to matter, ſtriking the auditory nerve: and that by means of an agent, the drum, or tympanum of the ear. That theſe ſenſations are from impreſſions which the nerves received is certain; for, if by rubbing my eye in the dark with my finger, by giving an impreſſion to the optic nerve, I can give a very vivid ſenſation of light and colours; the ſame if I put my finger into my ear and move it quickly; I can give a ſenſation of ſound: therefore, as there could no coloured rays of light come in the dark from my fingers: colours muſt be owing to the impulſe by which the rays of light, or fire ſtrike uponthe retina. The way to give theſe ſenſations to your eye by your finger is, to cloſe the eye and rub the internal canthus with it, being that part of the eye next the noſe.

As the picture of the object is painted upon the bottom of the eye, inverted, a great deal of diſcuſſion with philoſophers has taken place; why, we do not ſee objects inverted. If the ſenſation were conveyed to the brain, by this picture, and alſo the colours, we really would ſee them inverted. But, the picture, as repreſented upon the retina, does not impart the ſenſation as a coloured picture, but only each particle of light coming from the object, gives ſuch and ſuch a ſtimulus, in conſequence, ſuch and ſuch ſenſations are conveyed to the brain. If it was not ſo, how comes it that in a delirium, patients ſee objects with the ſame diſtinct viſion as when real. Now this can only take place from the diſeaſe giving a ſtimulus to the optic nerve, not the ſtimulus of a real coloured picture being painted upon the retina. Or upon writing, or reading a great deal (for, greatly I have laboured for the cauſe of truth and ſcience, yet little has been my reward) I have found little coloured images floating before my eyes; here the ſenſes were clearly not delirious.

That the eye does not diſtinguiſh colours, from theſe colours being painted upon the retina, appears, from many circumſtances: I ſhall mention one. I have long been in the habit of uſing green glaſſes; upon my firſt uſing them every thing appeared very green through them; but, by habit, that greeneſs went off and I could diſtinguiſh colours. Now there could only paſs through theſe glaſſes, according to the old doctrine, green rays, and that colour muſt have been painted upon the retina; therefore, this colour ought never to have gone off. But, according to my doctrine; as I ſuppoſe, the colours depend upon the force with which bodies reflect the rays of light upon the retina: therefore, from long habit, the retina receiving all its rays through this green glaſs, it was accuſtomed to receive the rays of light of a leſs impulſe; in conſequence, it adapted itſelf to diſtinguiſh the different impulſes of light, under this depreſſed, or impaired impulſe*.

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* It is from the repulſion of the particles of light; they paſs by each other without impeding each other as other bodies do, as in the light of two candles. See Count Rumford's experiments in the Phil. Trans.

MR. CAVENDISH,

I have made your Letter and this part of the work ſeparate; for this reaſon.—My former works clearly evincing the truth of my ſyſtem; therefore, your oppoſition to it can have no apology; for the truth muſt have appeared to every impartial reaſoner, without the aid of theſe new experiments, which are beyond all controverſy.

A LETTER

A LETTER

TO

HENRY CAVENDISH, ESQ.

SIR,

IN the year 1788, I wrote a letter to you and others upon the extreme errors and fallacy of the French ſyſtem, and alſo upon your two experiments, the firing inflammable and pure airs, and paſſing the electric ſpark through pure and atmoſpherical airs; proving, to the ſatisfaction of candour and common ſenſe, your very erroneous concluſions from theſe experiments: which letter you have never been able to refute.—I now addreſs to you another upon the ſame ſubjects, but I do not expect more candour from you towards this letter than the former; I am ſenſible there is a moſt powerful and illiberal combination formed againſt me and my ſyſtem. But, Sir, whatſoever be the arts, influence, and conſpiracy, time will unravel the whole.

Though this combination is ſupported by ſo numerous a body; a phalanx who flatter themſelves their names can command every thing, yet truth will and muſt prevail. If your opinions, experiments, and concluſions are juſt, then I am willing to ſtand condemned as cenſuring you unjuſtly; and in that cenſure, as being too confident of my own opinion. But I am not like you and your confederates, who ſkulk from inveſtigation. I do here ſeriouſly call upon the public to arraign us both at their tribunal, and to paſs their ſentence according to their juſtice. But, in fixing their judgment, I hope, they will carefully weigh the facts *pro* and *con*.

No doubt, Sir, you will call this letter preſumptuous; and if I do not prove the French theory to be *egregiouſly* falſe, and alſo, if called upon, ſhew that every mean, illiberal, and ſhameful artifice has been made uſe of to repreſs fair inveſtigation, I will agree with you that it is preſumptuous. When a man believes he has truth and juſtice on his ſide, when his opponents dare not openly refute, but take every method that cunning and art can invent to ſuppreſs fair inveſtigation, after they have been publicly called upon; then under thoſe circumſtances, Mr. Cavendiſh, I think, that I neither do juſtice to ſcience, truth, nor myſelf, if I do not ſtate my grievance to the world. I acknowledge that my language is harſh and pointed; but Sir, I appeal to your behaviour for its juſtification: I am aware of the great influence of this overbearing combination, I know that it is great and mighty, and, like many tyrants, has its janiſaries (the herd of reviewers) to ſtrangle its adverſaries.

As to theſe gentry, I think they muſt feel themſelves really diſarmed after my laſt heavy charges, which they were unable to anſwer, therefore theſe charges ſtill ſtand in all their force. But I am afraid, I am paying their feelings too great a compliment, for they are entrenched in braſs and ignorance, and deride the ſhafts of honor, juſtice, and truth: they now do not comment upon or analize my publications, but condemn *en maſs*, as they found they were ſailing in unknown ſeas beyond heir weak powers to fathom. But, Mr. Cavendiſh, I muſt apologize for ranking you in ſuch company, and alſo to myſelf in condeſcending to anſwer their illiberality. I know their judgment does not weigh an atom with the man of ſcience; but their writings have this bad effect, they can paſs that character upon a book as prevents its being read, particularly abroad; and when theſe opinions are echoed by ſuch men as you, Sir. I ſee Sir Charles Blagden has reſigned his ſecretaryſhip to the royal ſociety, and Mr. Tennant is elected into the council.

What muſt we judge from this? Is the former glad to be quit of his office, and the latter brought there to ſecond you in your ſchemes, of rejecting or admitting chemical papers, according as they refute or defend your ſuppoſed "*illuſtrious*" experiments? Sir, this is an age of trifling experimenters, men that have no genius, who expect immortality from their mechanical labours in their laboratory, though merely the reſult of chance. And when they come to philoſophiſe upon thoſe accidental experiments, they cannot carry their ideas beyond their laboratory; in conſequence, when their gigantic ſyſtems are applied to the various phenomena of nature, they form inconſiſtencies and abſurdities beyond the imagination to conceive, being the groſſeſt puerilities: theſe facts I publicly and openly declare to you and the public, and pledge myſelf for the truth, as being clearly demonſtrated in this letter, beſides my other publications.

This combination is not confined to England, it alſo extends to France. Dr. Prieſtley has wrote two papers lately, clearly refuting their hypotheſis, and directly calling upon them for an anſwer.—Certainly he ſtands as high as Mr. Fourcroy himſelf in the public eſtimation: but behold they are *mute*, they dare not enter upon inveſtigation, though they have reſumed their chemical purſuits. What muſt the candid world think? they principally reſt their abſurd hypotheſis upon inflammable and pure airs forming water. Dr. Prieſtley, and many others, have performed that experiment in a far more accurate and juſt way, and an acid is in the reſiduum along with water; this the French theoriſts knew; but to counteract this experiment, Meſſrs. Fourcroy, Vanquelin, and Sequin, performed it upon a large ſcale, and therefore not an accurate one; where they ſay no acid was produced but an air generated, formed of the condenſed acid and inflammable air, as Dr. Prieſtley ſufficiently proved after me. But why was not Mr. Lavoiſier and the other leading members of the French ſchool preſent at this ſuppoſed deciſive experiment; an experiment which they dare not repeat, nor will they admit of any contradiction. Their behaviour to M. La-Marck was alſo illiberal in the extreme, and unprecedented as gentlemen and philoſophers; though I have received worſe treatment. But can you flatter yourſelf that this behaviour will reign for any time? no Sir, "*For truth is mighty and muſt always in the end prevail.*"

You, Sir, have, I believe, been lately much in your laboratory in hoping to find experiments to defend this extraordinary ſyſtem. But I ſuppoſe you cannot find any, or otherwiſe you would not patroniſe ſuch trifling chemical papers, as the Phil. Trans. have lately produced: for a long time they were *mute* upon chemical ſubjects, and, I think it would have been more political to have continued ſo. Theſe chemical papers, I have fully ſhewn in this letter, to be futile and puerile.

Our aerial philoſophers ſeem to have got into the greateſt errors concerning the doctrine of combuſtion, ſuppoſing it is conducted by attraction: but combuſtion is clearly the ſeperating or breaking down the formation

tion of bodies, and not attracting or building up new ones. Thus fire enters into the integral ſubſtance of all combuſtible bodies, which bodies are thoſe which poſſeſs the greateſt quantity of fire; therefore when thoſe bodies are deſtroyed by fire, or have their fixed fire ſet looſe as actual, the compoſition of them is entirely broken down; from two cauſes. Firſt, As the fire made an integral part of the bodies, and ſecondly, as the free fire's great principle is repulſion: therefore, as the fire is let looſe, all the component parts of the burning body are repelled or forced from their chemical combinations by the repulſatory principle of fire; and unleſs chemiſts will introduce this great repulſatory principle of free fire into combuſtion, they never can account for the phenomena; for this great repulſion of fire is as certain an agent as chemical attraction.—Therefore our late chemical theories teaching that the air acts in combuſtion by attracting the ſuppoſed elements, carbone, inflammable air, or phlogiſton, from bodies when burning, is erroneous, as the air acts in combuſtion as the agent: it being a combuſtible body formed of fire, fixed air, and water, and its fire being ſlightly attracted to the fixed air and water, is therefore eaſily ſet looſe in the combuſtion, and then acts upon burning bodies as nature's great agent in ſetting looſe the combuſtible bodies' fire; and in reſpiration, putrefaction, and other proceſſes, this fixed fire is eaſily attracted from its union with the fixed air and water of the pure air uniting to the blood in its fixed ſtate.

But Sir, had that able electrical experimenter Mr. Cuthbertſon's experiments been given to the public in the tranſactions, it would have been a paper that would have done honor to the Society. But no, they were experiments which cut up your boaſted theory by the roots. Mr. Cuthbertſon has calcined different metals, by paſſing the electric ſpark through them both, *in vacuo*, in fixed air, and azote.

From whence comes the calcination, Mr. Cavendiſh? here is no oxygen-gas in the proceſs.—Sir, The explanation is obvious and clear, the electric fire was ſo intenſe as to ſet the fixed fire free, which gave the metal its ductility.* Will it not alſo ſet the fixed fire free which neutralizes the fixed air and water into atmoſpherical air, leaving the fixed air and water in the reſiduum?—This, Sir, both Dr. Prieſtley and Mr. Bergman have proved, and the true explanation of your experiments, with the electric fire, is, that when the pure air of atmoſpherical air is united with the pure air of the laboratory, both their acids will be actually condenſed in the experiments; this I have proved.

Why does Mr. Keir delay the publication of his experiments on metallic ſolutions? Thoſe who have ſeen the ſecond part, the firſt being publiſhed in the Phil. Tranſactions, ſay, that he has detected the groſſeſt errors in Mr. Lavoiſier's ſtatements: the experiments of men anxious to eſtabliſh a theory, ſhould always be received with great caution, and repeated. That thoſe ſtatements were groſsly erroneous I have proved in my different publications. See my Letter, and Chemical Eſſays. Then

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* The doctrine of the repulſion of the particles of fire to each other being now clearly eſtabliſhed; and the electrical fire paſſing through the metals being ſo very intenſe, ſee Dr. Van Marum's experiments, where he melted a conſiderable body of iron at one diſcharge; he alſo entirely calcined twenty-four inches of leaden wire, three eights of an inchin diameter, by one exploſion, therefore this immenſe heat of the electrical fire, will act ſtrongly in repelling the fixed fire of the metals, and ſet it free.

In my Letter, page 41.

'We cannot help making this remark here, that it is aſtoniſhing, that the moſt eminent chemiſts who have adopted the ſingular

Then I muſt again repeat, why, Mr. Cavendiſh, does not Mr. Keir produce his experiments which ſhew the *moſt groſs and palpable errors* in Mr. Lavoiſier's? No, that would endanger their grand ſyſtem: but will the world be deluded in this way! If theſe

'gular theory of the compoſition of the acids, ſhould not have more minutely enquired into it, before they gave us ſuch different opinions upon the ſame experiments. Mr. Metherie ſuppoſes, no nitrous acid enters into nitrous air, becauſe, he ſays, "though nitrous air is obtained from a ſolution of mercury in nitrous acid, almoſt all the acid is found in the ſolution."

'Upon the other hand, Mr Kirwan ſays, p. 38, "when a metal, or any phlogiſticated ſubſtance is diſſolved in the nitrous acid, this ſubſtance attracts the acidifying principle of the nitrous acid, and its phlogiſton is attracted by the nitrous baſis; and thus by a double affinity, the nitrous acid is in part decompoſed, and nitrous air formed."

'This ſuppoſed acidifying principle, according to him, is fixed air; according to Mr. Lavoiſier, is dephlogiſticated air; but according to Mr. Metherie's experiment, it is almoſt the whole of the nitrous acid employed in the ſolution. But I ſay, according to truth, it is all the nitrous acid, but what was taken up by the phlogiſton in forming nitrous air.'

'Mr. Lavoiſier, who firſt formed this theory, from obſerving the effects produced in a ſolution of the nitrous acid and mercury, ſays, "100 grains of dry nitrous acid, conſiſts of 64 grs. of dry nitrous air, united to 36 grains of pure air, deprived of its ſpecific fire." Mr. Kirwan, ſeeing in ſome meaſure the inſufficiency of this compoſition, ſays, 'In my opinion, 100 grains of pure, dry, colourleſs, nitrous acid, contain 38,17 grains of fixed air as its acidifying principle, 57,06 of nitrous baſis, and 4,77 of phlogiſton united to the nitrous baſis."—Both of them make the nitrous air above half the weight of the baſis; therefore, upon examining the acid after the ſolution, we ſhould have expected to have found, according to them, the other part of the conſtitutent bodies; agreeable to Mr. Lavoiſier, 36 of the pure air; and agreeable to Mr. Kirwan, 38,17 grains of fixed air. But unfortunately for theſe opinions, we find almoſt the whole of the pure nitrous acid. We certainly ſhould have expected, in adopting ſuch an extraordinary hypotheſis, From whence comes the calcination, Mr. Cavendiſh? here is no oxygen-gas in the proceſs.—Sir, The explanation is obvious and clear, the electric fire was ſo intenſe as to ſet the fixed fire free, which gave the metal its ductility.* Will it not alſo ſet the fixed fire free which neutralizes the fixed air and water into atmoſpherical air, leaving the fixed air and water in the reſiduum?—This, Sir, both Dr. Prieſtley and Mr. Bergman have proved, and the true explanation of your experiments, with the electric fire, is, that when the pure air of atmoſpherical air is united with the pure air of the laboratory, both their acids will be actually condenſed in the experiments; this I have proved.

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‘ hypotheſis,

theſe experiments had been in ſupport of it, the public would have had them many years ago.—But Sir, You *ſee and know*, that this boaſted ſyſtem is abſurd in the extreme, and that mine ſtands upon the ſolid baſis of immutable truth.

I could mention many other extraordinary ſimilar facts, but I ſhall tire my reader; I therefore will enter upon my ſtrictures on the different chemical papers in the Phil. Tranſactions.

'hypotheſis, ſo contrary to what chemiſtry had ever taught or ſug-
'geſted, that they ſhould have examined it more carefully. I leave
'my learned reader to make his own reflections, they appear ſo
'obvious; I ſhall make no other comments, the errors of the
'theory muſt appear in the moſt ſtriking light.

In my Chemical Eſſays page 72.

'Dr. Prieſtley ſays, vol III. p. 290, "Mr. Metherie found p.
"146, though nitrous air is obtained from a ſolution of mercury
"in nitrous acid, *almoſt all the acid* is found in the ſolution."—
'And Dr. Fordyce found *almoſt all the acid* in the ſolution of
'zinc in the vitriolic acid. Then can we have a doubt but both
'ſolutions are from the ſame cauſe: and not ſuppoſe the one
'from a decompoſition of the water, and the other from a de-
'compoſition of the acid. The nitrous acid has a ſtrong attrac-
'tion for the metals, the ſame as it has for calcareous earths; and if
'I add as much of the acid as to neutralize the earth and metals,
'and no more; and, if upon examination of them after the ſa-
'turation, I find all the acid, only allowing a little for what the
'nitrous air took up from the metal; can chemiſts form the moſt
'vague conjecture that all the acid is decompounded in the me-
'tallic *ſaturation*, as all the acid is found entire, and the ſame
'in both the ſaturations? But ſtill more forcibly to contradict
'ſo very *abſurd* an opinion; by paſſing the electric ſpark through
'nitrous air Dr. Van Marum reduced three-fourths of it into
'the nitrous acid.—(See a full explanation of this in my Letter,
'p. 32.) What led to this *extraordinary hypotheſis*, was the ex-
'periments of Mr. Lavoiſier upon mercury and which I have
'ſhewn can likewiſe *only* be accounted for by my hypotheſis.—
'See page 56 of theſe Eſſays.—Theſe are a ſmall part of my
obſervations upon the ſubject. See particularly my letter.

It requires no great obſervation to ſee, that all the exertions and objects of our preſent chemical philoſophers are, to defend the abſurdities of the French theory; ſeeing I have oppoſed it by ſuch ſtrong arguments, proving inconteſtibly its very weak, unſtable, principles: therefore there appears clearly a general combination to ſupport its tottering baſis; honor, candour, and juſtice are all to be ſacrificed in the conteſt. That theſe obſervations are juſt, will clearly appear from the following ſtrictures.

Obſervations on DR. PEARSON'S *Experiments.*

WITH SOME OTHER REMARKS.

Dr. Pearſon has written a moſt extraordinary paper, in the Phil. Tranſactions, endeavouring to ſhew that water is decompounded by paſſing the electric fire through it. I muſt make this obſervation, that ſuch futile experiments and reaſoning do not deſerve any anſwer; however, I ſhall not honor them with a full inveſtigation, but only make ſome curſory remarks.

In the firſt place I muſt make this general obſervation, that Dr Pearſon's experiments and manner of reaſoning are always looſe and unſatisfactory. Upon the Dutch experiments being publiſhed, ſome of the ableſt electricians and chemiſts endeavoured to repeat them, but when performed in an accurate manner, they found that they could produce neither inflammable nor pure airs: Dr. Pearſon thinks himſelf more lucky.—But muſt not my reader be aſtoniſhed when he finds theſe experiments were performed with a braſs conductor, of a large ſurface, and that there was a ſenſible eroſion or calcination of the metal, ſo that the inſtrument would only do for a ſhort time; and moreover, that the water through which the electric fire paſſed, was expoſed to the open air.

In page 146 he ſays, " By repeated diſcharges " there is an impreſſion made in the braſs tube, in " the part where the diſcharge paſſes through it, " and at laſt a ſmall hole is made in that part. On " this account the ſame mounted tube cannot ſerve " for producing a large quantity of gaz."

Now, are Dr. Pearſon and his friend Mr. Cavendiſh ignorant, that water being ſo intenſely heated, as it muſt be in this experiment, would calcine metals and produce inflammable air; and alſo that when water is heated, it will expel the pure air it contains, and alſo that water which has got its air expelled, will attract pure air from the atmoſphere?* The water became opake in the experiment, clearly ſhewing, (ſee my experiments) that as it parted with its air, that the bodies which it held in ſolution were precipitated. The Dr. ſays, he did not perform the experiments with water which had got all its air expelled by former experiments, leaving that to others.

But I ſhall aſk, *even his candour or yours, Mr. Cavendiſh*, if that would not have been the only juſt way of performing the experiment, as being water which had given out all its pure air? for performing it with water which had not given out all its air by the experiment, only proves that the electrical fire acts as common fire in expelling the air from water, the ſame as ſteam of water in calcining metals, and forming, by that means, inflammable air. But we wanted not a paper of the Phil. Trans. to prove to us that the electrical fire acts as common fire, for all the world knew before that it would melt and calcine metals, Dr. Van Marum melting a conſiderable body of iron at one diſcharge of the battery.

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* Dr. Pearſon ſays, there was " Always left a reſidue of at " leaſt one fourth of its bulk on paſſing through it the electric " ſpark, even when water was uſed, which had been free from " air by boiling or the air pump." Then can there be a queſtion, Mr. Cavendiſh, from whence the pure air proceeded, as it contained ſo great a proportion of azote? if it had been from the decompoſition of water it would have contained no azote according to your own hypotheſis.

And what a quantity of electrical fire was required to produce a ſmall quantity of gas! for the Dr. ſays, " My calculation alſo, that 35 to 40000 " diſcharges were requiſite to produce one cubical " inch of gaz from water, containing its uſual " quantity of common air."

Dr. Pearſon's paper then is a moſt puerile performance. But I ſhall give him acknowledgment for this ſagacity, that he knew the only proper way would have been to have proved that the ſame water would have done for the experiment, again and again, and that he would accordingly try this part of it with the greateſt attention; as the ſuppoſed decompoſition of water greatly reſted upon it. But he is obliged to ſay, " It may now be " expected, that I ſhould have made the experi- " ments with this apparatus on diſtilled water " freed from its air, not only by long boiling, or " the air pump, but by paſſing through it ſeveral " hundred electrical diſcharges. It would alſo " have been, to ſome perſons, more ſatisfactory, if " the experiments had been made upon a larger " ſcale, ſo as to have produced the combuſtion of " a much larger quantity of gaz, and conſequently " have produced a greater quantity of water."

The Dr. has been performing theſe experiments nearly two years, for he ſays, " From my journal " of the numerous experiments, made during the

" courſe of nearly two years, I ſhall ſelect thoſe " which will ſerve to explain the nature of the " proceſs, and ſhow the power of the plate electrical machines; and I ſhall particularly relate " thoſe experiments which afforded the moſt uſeful " reſults concerning the nature of the gaz obtained."

Therefore we have a right to ſuppoſe thoſe experiments were ſelected to prove this abſurd doctrine of water being compoſed of pure and inflammable airs, for, upon this idea reſts the whole fabrick of the French theory. But I muſt again obſerve, Mr. Cavendiſh, was it neceſſary in this experiment to have one of the metal conductors formed of a metal that is ſo eaſily calcineable.—If theſe airs proceeded from the decompoſition of water, why might not two golden wires have done; the one to carry and the other to receive the electric fire; as they perform the circuit as well as any other metals, but he knew they would not.—In ſhort I might make fifty other objections to theſe experiments, but I have given them more attention already than they deſerve.‡

Mr. Cavendiſh, I before addreſſed a letter to you and others; and I hope, ſatisfactorily proving your fallacious theories to every *rational* chemiſt. I then told you that your *ſuppoſed* great diſcovery of nitrous acid being formed of pure air and azote, by the electric ſpark, was fallacious; that the cauſe of the nitrous acid being formed in that experiment, was, from the pure air of the atmoſphere, and the factitious pure air of the chemiſts being burnt ſo intenſely as to precipitate both their acids in a condenſed ſtate: but, that in common combuſtion, their acids are left in an aerial ſtate, as fixed air; and that atmoſpherical air is neceſſary in the proceſs, as its fire is more eaſily ſet looſe by the electric ſpark; fully diſcovered by the electric ſpark burning it into fixed air, proved by Prieſtley, and Bergman. Mr Kirwan ſays, " It is impoſſible " to deny all credit to thoſe who aſſerted that " lime-water was precipitated by taking the elect-" ric ſpark in common air, though it did not " ſucceed with him (Mr. Cavendiſh) either from " his uſing an inſtrument of different power " from that uſed by others, or air phlogiſticated " by a different proceſs "

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‡ It certainly muſt be a dying cauſe when they can bring only ſuch futile, weak, experiments to defend it.

§Now the pure air of atmoſpheric air's fire being eaſily ſet looſe, acts upon the artificial pure air of chemiſts, ſo that there is ſuch an intenſe combuſtion as to precipitate both their acids into a concentrated ſtate, as the nitrous acid; ſo that azote has nothing to do in the proceſs ‡

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‡ In my letter page 126.—'We come next to Mr Cavendiſh's ' important experiment in forming the atmoſpherical and de-' phlogiſticated airs into the nitrous acid; but we ſhall give a ' very different explanation of it, from what Mr. Cavendiſh has ' done. I have all along ſuppoſed that reſpirable airs are formed ' of an acid, fire and water; and that the atmoſpherical air is a ' very different air from the artificial empyreal air of the chemiſts: ' the latter not at all ſupporting vegetable life, and likewiſe ' animal life not being able to injure it completely, as animals ' died in it when it was better by the nitrous teſt, than the ' pureſt atmoſpherical air; (ſee my thoughts on air.)

' Hence in Mr. Cavendiſh's experiment, when the atmoſphe-' rical and artificial empyreal airs are mixed, there is a quantity ' of actual fire thrown into them. We have before proved, that ' fire will decompound reſpirable airs, and according to the in-' tenſity of the fire, the air will be accordingly decompounded. ' If

§ I need not inform my reader that I have proved, by fifty experiments, that pure airs are formed of an acid, fixed fire, and water, ſo as to form an aerial neutral ſalt.

I ſee, upon the continent, a German chemiſt, M. Gitting, has got into the ſame miſtake in the burning of phoſphorus, for he found that atmoſpherical air was neceſſary to phoſphorus burning in a cretain temperature. But, Sir, the true explanation is, from the pure part of the atmoſpherical air, being neceſſary in the proceſs, as its fire is more eaſily ſet looſe, and not from the impure part of it; this I have fully ſhewn in my publications.

Sir,

'If it is the common combuſtion, ſuch as wood, charcoal, &c. 'the air will be left in the ſtate of fixed air; but if it is a more 'intenſe heat, ſuch as ſulphur, phoſphorus, metals, &c. the air 'will be decompounded into an acid and water.

'Now the electric heat is moſt intenſe, it being capable 'of producing a phenomenon that we cannot imitate by any 'chemical proceſs. Metals are only melted by heat; the electric 'fire is ſo wonderfully intenſe, as to diſſolve iron in one diſcharge 'of the battery. Dr. Van Marum melting a conſiderable body 'of iron at one diſcharge; he likewiſe entirely calcined 24 'inches of leaden wire, thee-eights of an inch in diameter, by 'one exploſion. This ſhews a degree of heat we cannot imitate. 'Our heat that we can apply, requires a long continued application, but this is inſtantaneous. Therefore when empyreal air 'is acted upon by this wonderful heat, we ſhould expect it 'would decompound the air to an acid and water.

'This theory is moſt ſtrikingly ſhewn, in firing inflammable 'air and empyreal air. If there is a full proportion of inflammable air, there will be ſuch a degree of fire ſet looſe, as to 'fly off with the acid, as we have before explained; but if there 'is a leſs proportion of inflammable air, ſo as not to produce ſo 'great a heat, the acid will be left in the reſiduum. A ſimilar 'phenomenon will take place in the burning of the reſpirable 'airs in the electrical fluid. If the electrical fluid is conſiderable, 'and the empyreal air very rich, part of the acid will be carried 'off in the ſame manner as when burned in inflammable air. But 'if the electrical fire is not ſo conſiderable, and the empyreal 'air not ſo rich, far more of the acid will be left in the reſiduum. 'This will account for the different reſults between Mr. Caven'diſh's and Dr. Van Marum's experiments; the latter uſing a 'greater quantity of the electrical fluid, and a richer air than 'the former.

Sir, for the ſake of juſtice, truth, honor, and ſcience; do not, by your great influence, embarraſs and overpower candid inveſtigation. I ſhall aſk you and ſome others a plain and obvious queſtion. How comes it that the oxygen-gas, or pure air made in the laboratory, will not ſupport animal life? Mr. Scheele, Dr. Prieſtley, and others, in breathing it, could not reduce it ſo far as to bring it lower than the ſtandard of pure air. Hear, O common ſenſe! the reaſon aſſigned for it

by

' Mr. Cavendiſh ſuppoſed that the acid came from the phlo-' giſticated air; this aroſe from their theories, which ſuppoſed ' that there was no nitrous acid in empyreal air. But he found ' that he could not obtain any nitrous acid in this proceſs, when ' he uſed only phlogiſticated air, and he could obtain ſome when ' he uſed empyreal air. This he imputed to the impurities of ' that air: however, there is more of it decompounded than can ' be accounted for by that cauſe. But when he mixed the ' atmoſpherical air with the artificial empyreal air, he then could ' form them into the nitrous acid with the electrical fluid.

' I have all along (in my publications) ſhewn the atmoſpheri-' cal air to be a very different air from the artificial reſpirable air ' made by chemiſts. The former is made by nature's own de-' licate hand; her ingredients being water, fixed air, and the ' ſun: the latter being made in a laboratory, with mineral ' acids, water, and earth, or ſalts, forced into an aerial form by ' an intenſe fire. The one will ſupport both animal and vege-' table life, but the other will ſupport neither of them; ve-' getables dying immediately in it, and animals long before ' they have injured it, even when it is in a far purer ſtate than ' the beſt atmoſpherical air.

' Hence in this experiment of Mr. Cavendiſh, the electrical ' fluid is not able to burn ſo conſiderably in empyreal airs, ſo as ' to reduce them, except the atmoſpherical air makes a part of ' them. By the atmoſpherical air, the electrical fire is ſo ignited, ' as then to operate forcibly upon the artificial empyreal air, and ' to produce ſo ſtrong a degree of heat, as to decompound the ' airs.

' This

by our wonderful French chemiſts, that it produces uch an inflammation upon the lungs as to ſtop re-

‘This artificial empyreal air, we have likewiſe all along ſhewn, to be of a ſtrong combuſtible quality, burning with partial exploſions (according to Dr. Prieſtley as if it was full of ſome combuſtible matter). But by adding a greater quantity of phlogiſton or concentrated fire to it, I have made it ſo inflammable as to explode all at once! (ſee my thoughts on air.)

‘Theſe two airs then, viz nature's atmoſpherical air, and the high concentrated artificial air of chemiſts, being mixed together, and expoſed to the influence of the electrical fire, will be decompounded, producing the nitrous acid; but the acid is not produced from the decompoſition of the phlogiſticated air. For take only the empyreal part of the atmoſpherical air (which is eaſily done by expelling the air from water; and then expoſing the water to the atmoſphere, and then expelling the air again). After that, mix it with the pureſt artificial empyreal air, and there will be the ſame phenomena.

‘Or take the phlogiſticated part of the atmoſphere only, and mix it with the pureſt empyreal air, and there will not be theſe phenomena. No doubt ſome acid will appear, but it will be no more than what would have been, if the phlogiſticated part of the atmoſphere had not been added to the artificial empyreal air; for the higher the concentration of fire in aerial bodies, the greater difficulty there is in decompounding them. Thus in animal reſpiration, I can decompound the whole of the empyreal part of the atmoſpherical air, and only about one third of the artificial empyreal air, and that with difficulty; ſo that inſtead of its being a more luxuriant air for animal life, it is the reverſe. This is diſcovered by your breathing in it; and very ſenſibly in the reſpiration of mice; that delicate animal is ſeen to have the greateſt difficulty of breathing in it from the firſt, and dies before it has fully injured it.

‘But even inflammable air may be breathed and decompounded; the immortal Scheele reducing a great quantity of it to foul air, by breathing it alone. Therefore if the artificial empyreal air is expoſed to the electrical fluid itſelf, it will in ſome meaſure be acted upon as we might expect. But Dr. Prieſtley has ſhewn us long ago, that the empyreal part of the atmoſphere may be all decompounded by the electric ſpark.

‘This then is the true explanation of the experiment; and the acid which is depoſited in the experiment, comes from the empyreal air.’

ſpiration; can theſe men, with their puerile reaſons, pretend to be philoſophers! Every phyſician muſt have ſeen the higheſt inflammation produced upon the lungs in diſeaſes, and yet the patient ſtill breathes and lives.¶

But this vital pure air, which is ſo neceſſary to life, ſhall, in an exceſs of it, kill, in a few minutes. Examine a mouſe which has died in it, and ſee its lungs if they are at *all inflamed.* No, they will be found in the *identical ſame ſtate as if it had been reſpiring azote.* When any combuſtible body burns in it, as Dr. Prieſtley ſays, "It burns with crack-"lings as if it was full of ſome combuſtible "matter."

The common air of the atmoſphere, I have proved by inconteſtible experiments, is formed of fixed air, fire, and water, but the pure air or oxygen-gas of the laboratory, of the mineral acids, water and fixed fire, and, in conſequence, it retains its fire ſo ſtrongly that the blood finds a difficulty of decompounding it; ſo that an animal will be a longer time in decompounding a given quantity of it than a quantity of pure air of the atmoſphere that has identically the ſame quantity of oxygen-gas in it, nay twice as long as Dr. Prieſtley acknowledges, and it even leaves it in as pure a ſtate as the atmoſphere.* Liſten again, you *profound* chemiſts. An animal ſhall die in it *preciſely* at the time it brings its ſtandard to atmoſpherical air. (ſee both the experiments of Prieſtley and Scheele) Would it not, you profound conjurors, at that moment become perfectly ſalutary? otherwiſe, when the

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¶ I attended a patient who had a red hot iron run into his lungs, which produced the higheſt ſtate of inflammation that could be conceived, and yet the patient ſurvived it. I think I may hazard the idea that it would be a greater one than the breathing of oxygen-gas. Inflammation of the lungs is attended with the greateſt pain. Was Mr. Scheele's and Dr. Prieſtley's lungs, upon breathing this air, in pain? No, upon leaving off breathing this air they were perfectly well. It muſt be a curious kind of inflammation that kills ſo *immediately*, and diſappears ſo *immediately*.

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* In my Chemical Eſſays, p. 39. ‘As the forming fixed air into dephlogiſticated air, is certainly a proceſs by which we may accurately judge of the formation of dephlogiſticated air, I have paid a particular attention to it; but I will not give the minutiæ of the experiments, as many have done. If the public, after the uſage I have received, gets a detail from me, they may be ſatisfied. To other philoſophers the Philoſophical Tranſactions are open for their long hiſtory of dry experiments: *but I hope the day of reckoning and retribution will come.* That fixed air, when ſoluted in water, will, by the action of the ſun, form pure air, has been proved by Dr. Prieſtley and others.—As my theory ſuppoſes that fixed air and water, united to the rays of the ſun will form pure or reſpirable air; therefore to ſhew whether my theory, or Mr. Lavoiſier's is juſt, I made a number of experiments. I took freſh diſtilled water, and expoſed it to a ſtrong heat, without getting any kind of air from it; I then added to it a quantity of fixed air, which it readily abſorbed, marking the quantity; after that, I expoſed it to the rays of a hot ſun. But as tranſparent bodies are well known to admit the rays of light to paſs through them, without arreſting or ſtopping their paſſage, I added an extraneous body; any will anſwer, ſilk thread, or a dead leaf, dried ſtraws, &c. which have an attraction for the air; and by this means I found a great quantity of pure air riſe to the top of the decanter which contained the water; after that I expelled all the air from the water by heat, and I obtained rather a larger volume of pure air, than of the fixed air uſed; and not an atom of the latter. I found that two things were to be attended to in thoſe bodies, which were added to the water, to aſſiſt the proceſs; viz. they ought not to be tranſparent, and ought to have a kind of an electrical repulſion to water, ſo that the air may ſtand upon them in diſtinct globules: I found glaſs bodies, for theſe reaſons, improper, and likewiſe linen thread which had a great attraction for the water.

‘It

the animal was brought into the open air, it would neceſſarily *die from its inflammation.* How comes it that animals die immediately (the ſame as in the moſt noxious air) in Dr. Prieſtley's dephlogiſticated nitrous air, though it will allow a candle to burn in it with ſuch an enlarged flame? The reaſon is, as I have ſhewn, its fire is ſo fixed and concentrated

'It is remarked by philoſophers, that green animalcules appear when the water gives out air in the greateſt abundance.—'This appears to be from their arreſting the ſun's rays, and attracting the air in the water, the green ſeeming to be the beſt 'colour; and which nature indeed, makes uſe of in the great 'vegetable world, in attracting the rays of the ſun. When 'theſe green animalcules appear in the water, by gently adding 'fixed air, you may continue on, for a long time, the proceſs of 'generating pure air.

'The globules of air, when they are ſeen ſtanding upon the 'ſilk, &c. appear little at firſt, but gradually grow bigger; the 'ſilk reflecting the rays, and by that means they enter the globule, warming it, and ſaturating the air, growing larger, and 'as it were generating or forming pure air, by the fixed air attracting the rays, and ſaturating itſelf, by neutralizing them 'along with the water. We ſee moſt of the ſaline productions 'of nature are formed of acids, an alkaline ſalt, (which I ſuppoſe fixed fire) and water, and forming regular cryſtals. In 'the vegetable kingdom, moſt of the bodies belonging to it are 'formed of acids, fire, water, and earth; in bitumens, oils, &c. 'a more concentrated fire is neceſſary with acids, fire, water, and 'earths; and the different concentrations, combinations, and 'proportions of theſe bodies make the different bodies upon the 'earth, without running into the wild ſpeculative opinion, that 'all bodies are formed of airs. No airs are formed of them.'

From the GENTLEMAN'S MAGAZINE, for *December*, 1794.

TO SIR JOSEPH BANKS.

SIR, *Carliſle, Oct.* 15.

'I am much obliged to you for your politeneſs in ſaying, "I "ſhall readily receive any paper you are pleaſed to ſend me; and, "if the doctrines it contains are not, in my opinion, contradicted "by ted that animals cannot decompound it, being formed of the pure nitrous acid and phlogiſton; nay, Dr. Prieſtley formed it of ſuch a high proportion of phlogiſton, or fixed fire, that it exploded: I have formed

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“ by the reſult of experiments already made, or which may be “ tried after receiving it, for the purpoſe of bringing them to “ the teſt of reaſon and truth, I will moſt readily preſent it to the “ Royal Society.” ‘ I, therefore, ſend you the experiments, in order that you may have them tried, and ſee whether they are juſt or not.

‘ After the numerous experiments which I have made (and publiſhed in my different Works) by expoſing water impregnated with fixed air to the influence of the ſun, and thereby producing *pure* air; and likewiſe after the experiments of Dr. Prieſtley, in which water produced air by diſtillation; a very important queſtion occurs, viz. to what cauſe are theſe productions of pure air to be attributed?

‘ To elucidate theſe phenomena, I made the following experiments. I took ſnow-water, which had been previouſly diſtilled ſeveral times, taking care to prevent its contact with the atmoſphere, ſo that it would yield no air, either by the proceſs of expoſure to the ſun, or by diſtillation. I then impregnated it with a ſmall quantity of fixed air; after that, I added a little earth which had been precipitated from ſpring-water by the proceſs of boiling; then corking them carefully in a bottle, and ſhaking them occaſionally till the earth was ſoluted by the fixed air and water. After that, I put the ſolution into a ſtill, and let it undergo the ſame proceſs of diſtillation which Dr. Prieſtley ſpeaks of in his laſt publication on this ſubject; and I got from it a quantity of air, partly pure and partly azote. And, as the airs were generated, the water depoſited the earth which it had held in ſolution. And by repeated diſtillations it would yield no more air till fixed air was added to it again, and which ſoluted again the precipitated earth. And I found that if the ſolution be expoſed to the light of the ſun for ſometime previous to the diſtillation, the experiment will be aſſiſted.

‘ I have likewiſe found that water, which had undergone the action of the ſun, and had produced air, as in the experiments of Sir Benjamin Thompſon, will do equally as well for the above experiment as ſnow-water: for, after it has boiled, it will be found to poſſeſs no air, its power having been exhauſted by

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formed it of ſuch a high proportion of phlogiſton, that it has allowed the flame of a candle to extend to twice its uſual volume, that is, more of the fixed fire or phlogiſton of the air was burning at the ſame time.

Now

the air which it had already generated by the action of the ſun. But, if fixed air be added to this water, it will again ſolute the earth which had been precipitated from the water by the proceſs. And, if this ſolution be either diſtilled, or expoſed to the ſun, pure air and azote will be again generated, the earth precipitated, and the fixed air diſappear.‡

'Now I think, Sir Joſeph Banks, theſe experiments require no comment; it is unneceſſary to ſay from what cauſe theſe phænomena proceed. But I ſhall ſay nothing upon that head; I only ſend you the experiments that they may be repeated, and their validity aſcertained.

'The Dutch chemiſts have repeated an experiment of mine, in which, from expoſing ſulphur and iron to heat, I found that inflammable and vitriolic acid airs were generated: their reſults are ſimilar to mine. But I have carried the experiment farther than they have done. By expoſing the ſulphur and iron to a greater, more rapid, and more continued heat, I have produced a greater combuſtion, and formed them into a vitriolated iron. But this proceſs requires attrition as well as great heat; which I contrived to give it by a heated iron or glaſs peſtle, moving it rapidly at the time. Now, as the ſulphur and iron were pure, and perfectly dried, there being no water, acid, nor pure air, in the proceſs; and as great heat and flame were generated, therefore I think philoſophers will agree with me, that the ſulphur and iron were decompounded of their fixed fire, of that fixed fire which formed the one into ſulphur and the other into a metal, as the reſiduum was a vitriolated iron.

'I ſhould hope that what I have ſaid in my Chemical Eſſays muſt ſufficiently prove, that in Dr. Fordyce's late experiments, publiſhed in the Philoſophical Tranſactions, the vitriolic acid was

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‡ Will it be believed, Mr. Urban, that this experiment, from which a knowledge of the origin and formation of the atmoſphere may be deduced, could not obtain a *reading* before the moſt learned and reſpectable Society in the world, which, for a ſeries of years, has made the ſtudy of air one of its moſt peculiar and moſt intereſting objects? And without deigning to aſſign a reaſon, though much importuned.

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Now I think this view of the phenomena ought clearly to explain them. Nitrous air will ſooner ſet fire to phoſphorus than the artificial pure air, for

the principal calcining body, aided by the water; and that, when he added the alkaline ſalt to the ſolution, it attracted the acid from the calx. But to prove it more clearly—

'If a calcareous earth be ſoluted in the vitriolic acid and water, and precipitated by the ſame alkaline ſalt, or *kali purum*, it will be precipitated as lime; and, in both theſe ſolutions, the acid is required to be mixed with water. But, if the calcareous earth be precipitated with the mild alkali, it will be thrown down as calcareous earth. And, that the calcareous earth was ſoluted or acted upon by the acid, is clear from its fixed air being expelled; and it is equally clear that this caſe is ſimilar when metals are ſoluted or calcined by acids and water; for, their phlogiſton is expelled; the water in one proceſs going to the formation of fixed air, and in the other to that of inflammable. But, if any doubt ſtill remains, that the acid and not the water is the calcining body, let the ſolution be expoſed to a ſtrong heat (which is one of Dr. Prieſtley's experiments) the calx will be precipitated; and if carefully examined, being previouſly well waſhed in water, in order to waſh away any acid that does not make a part of the calx, it will be found to be formed of an acid and the earth of the calx, together with a ſaturation of water. What muſt we think of that theory which conſiders water as the calcining body when the vitriolic and marine acids are uſed, and the nitrous acid when that is uſed in the proceſs? But I have found that, if the dephlogiſticated marine acid be employed in the proceſs of calcining metals, even though it be mixed with water, marine acid air is produced, and not inflammable air; which is owing to the marine acid having a part of its phlogiſton taken from it; therefore, it attacks the phlogiſton of the metal with more force or violence, and conſequently greater heat is produced, and they form the marine acid air. The nitrous acid does the ſame, having likewiſe a ſtrong attraction; for phlogiſton (a well-known fact) will attack the phlogiſton of the metal with violence and force, producing ſo great a degree of heat as to form the nitrous air, which is an acid one, the ſame as the marine acid air; for, they both turn the vegetable juices red, being airs containing more of the acid and leſs of the phlogiſton, which forms the inflammable air principally; though all theſe airs have water for their baſis.

'And

for its acid acts in decompounding the fixed fire of the phoſphorus; and all airs formed from the nitrous acid do the ſame. Phoſphorus will ſooner be ſet on fire by the weight of the atmoſphere being

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'And farther, to render this doctrine ſtill more clear. If the nitrous acid be not ſtrong, and if the ſolution be made in a veſſel ſurrounded by a freezing mixture, the acid being gently added to the metal, they will only produce a phlogiſticated air. This is the caſe when tin is added to a neutral ſolution of tin in the nitrous acid; it is calcined, a calx is thrown down, and an imperfect nitrous air produced, which is ſo much phlogiſticated as to have its acid neutralized ſo as not to affect the vegetable juices.

'Or, in the ſolution of zinc in the nitrous acid. if the acid be gently added in a freezing ſituation, they will generate an inflammable air that will explode. By producing as little effervſcence as poſſible, and conſequently little heat, the acid gets fully ſaturated with phlogiſton.

'The action of the acids upon metals is exactly the ſame as that of fixed air, or aërial acid on lime, which is an earth ſaturated with fire; but fire more looſely concentrated than in metals. And it is worthy of remark, that the aërial acid will not expel the fire of the lime without the aid of water, but by their joint influence or attraction for the earth of the lime they will precipitate the fire; exactly as the vitriolic acid and water will expel the fire in acting upon the metals; but the metallic fire is expelled in a fixed ſtate as inflammable air. For a more full elucidation of this doctrine I muſt refer to my former publications.

'If the action of acids upon phlogiſton is managed in a gentle way, without producing great heat and effervſcence, they will unite without forming airs. As, for inſtance,

'If the volatile vitriolic acid be added to iron nails (which is one of Dr. Higgins's experiments, ſee p. 49 of his laſt publication) they will generate no air, but the acid and the phlogiſton of the iron will form a ſulphur; which ariſes from this, the acid being phlogiſticated, its activity for additional phlogiſton is partly blunted, and it unites to it in a very gentle manner. But I have found, if this mixture be made in a veſſel expoſed to great heat, that both inflammable and vitriolic acid airs will be generated, And it is from the ſame cauſe that the tin produces ſo high a phlogiſticated air when freſh tin is added to a ſolution of tin in the nitrous acid; for, the acid leaves the calx to attack the being taken from it, as its volatile parts are ſooner evaporated. Inflammable air will alſo aſſiſt its burning by diſſolving it.

But one of the moſt ſtriking of the French experiments, is,* that fixed air and water will make it burn better than atmoſpherical air. I have fully ſhewn (ſee my Chemical Eſſays) that phoſphorus has a great attraction for fixed air and water, and that it is this attraction makes it burn in atmoſpherical air, from its attraction for its fixed air and water, and alſo from its attraction for acids and water; therefore it will burn in acid air, as the French chemiſts have ſhewn. It burned in fixed air and water, ſo well, that they ſay, "Which light increaſed to a greater degree of brilliancy than "would have been exhibited in atmoſpherical air."

But this phenomenon was not owing, as they ſuppoſed, to the pure air of the water which the fixed air took along with it in paſſing through the water; for, if the fixed air paſs through mercury the ſame combuſtion will take place, provided the phoſphorus is expoſed to a little water along with fixed air. By their joint influence, they penetrate the phoſphorus and ſet its fire looſe; and they will

the phlogiſton of the freſh tin; but it leaves the one to attack the other in ſo gentle a manner, that little heat or effervescence is produced, and the acid gets its full ſaturation before it is ſufficiently aërilized, to produce an air that will admit of a candle burning in it with an enlarged flame. But, if this experiment be made in a veſſel ſurrounded by a freezing mixture, it will get ſo full a ſaturation of phlogiſton as to form inflammable air which explodes. Surely this is a connected chain of facts which cannot be miſunderſtood. I am, Sir, your moſt obedient, humble ſervant,

'ROBERT HARRINGTON.'

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* See the preſent French chemical publication, where there is a number of experiments upon the burning of phoſphorus.

both be found to be imbibed by the phoſphorus producing the phoſphoric acid; and, as a death blow to our French chemiſts, no charcoal or inflammable air is produced. The true explanation is this, phoſphorus is formed from the phoſphoric acid and fire, which are very looſely combined, ſo that they are eaſily ſet looſe by the joint powers of water and fixed air, which being attracted by the dry concentrated phoſphoric acid, in conſequence, becomes fluid.

That fire and the phoſphoric acid form phoſphorus is ſufficiently proved by experiments.— See my thoughts on air, where, by expoſing the phoſphoric acid with a ſmall proportion of water to a long continued heat, I formed the acid into phoſphorus; and this ſhews how all the pure airs of the laboratory are formed; only the nitrous acid when united to the calx of lead, attracts a far leſs concentration, forming what they call oxygen-gas; which, "Burns as if it was full of combuſtible "matter." But the phoſphoric acid, with the calx of lead, form inflammable air.

But the pure air of the atmoſphere does not burn with theſe cracklings. To make the experiments, take the ſame proportion of pure air made from nitre, and mix it with an equal proportion of azote, as they ſay atmoſpherical air is formed of, and this air will burn with thoſe cracklings which are partial exploſions; and alſo, if breathed by an animal it will not be able to leave it in an impure ſtate; nay vegetables immediately die in it. But in the pure air of the atmoſphere, none of theſe phenomena take place. It is hard, after what I have ſaid, ſtill to have theſe *abſurd doctrines* to expoſe. As our preſent aerial philoſophers catch at any thing ever ſo inſignificant: they may ſay, that, in the reſpiration of pure air from nitre, why an animal cannot perfectly injure it, is owing to the fixed air produced; but Dr. Prieſtley found, that, when a mouſe breathed it over lime water, it made no difference.*

Obſervations

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* In Natural Philoſophy, vol. 5.—“ But to make the experiment in the moſt unexceptionable manner that I could contrive, I, in the next place, got two mice, of nearly equal ſize, and put them into exactly equal quantities, viz. about five ounce meaſures, of the ſame dephlogiſticated air (the meaſure of its purity, with two equal quantities of nitrous air, being 0.24) in nearly equal and ſimilar glaſs jars, one ſtanding in lime water, and the other in common water. Both the mice continued in this ſituation ſomething more than two hours and an half, after which the air which had been confined by lime water appeared to be reduced in the proportion of 9 to $5\frac{1}{4}$ the meaſures of the teſt being 0.96; and the air which had not been confined by lime water was diminiſhed in the proportion of 9 to $6\frac{3}{4}$, the meaſures of the teſt being 0.98. Both the mice, though kept pretty warm, laboured alike with a difficulty of reſpiration, ſome time before I put an end to the experiment. In the courſe of it I agitated the lime water a little now and then, in order to make it abſorb the fixed air the better, by admitting freſh lime water to the air that had been reſpired.

“ It appears from this experiment, that the air confined by lime water was both diminiſhed and phlogiſticated exactly like that which had been confined by common water, by the reſpiration of mice of equal ſize, in the ſame time. The diminution indeed was, at firſt, a ſmall matter greater in the air confined by the lime water; becauſe the common water did not imbibe the fixed air ſo readily; but this made no apparent difference with reſpect to the mice, and the next day the two portions of air were found to be as nearly as poſſible of the ſame dimenſions and of the ſame degree of purity.

“ In the preceding experiments, and ſeveral others which I made about the ſame time, I found that mice would not live in deplogiſticated air till they had completely phlogiſticated it. though they lived longer in it than, in proportion to its purity, with reſpect to common air; and for this I cannot aſſign any ſufficient reaſon. I had once imagined that this was owing to my being obliged to make the mice paſs through a quantity of water, by which the air was confined; but I put a mouſe through the ſame water into a quantity of common air, and

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Obſervations on Dr. W. Henry's Paper.

Now, Mr. Cavendiſh, as you command the key to the chemical department of the Phil. Trans. let us conſider the next paper that you have patronized; it equally bears the ſame marks of puerility as Dr. Pearſon's. Indeed after what has been ſaid, and at this advanced period of the inveſtigation of airs, it appears moſt extraordinary. But Sir, I will pay your ſagacity that compliment that you muſt ſee the extreme futility and abſurdity of theſe papers, yet they anſwer the purpoſe of that policy you have adopted in your combination againſt my plain and rational theory. But to the examination. Dr. Auſtin, who certainly was one of the firſt experimenters, has clearly proved from his experiments, that, in paſſing the electric ſpark through the heavy inflammable airs, they were expanded to twice their bulk, and from thence he concluded that, the charcoal which formed a part of theſe airs was decompounded into the light inflammable air and azote; for, in firing theſe heavy inflammable airs with pure air, before the paſſing the electric ſpark through them, there was a conſiderably greater quantity of fixed air in the reſiduum, than after the electric ſpark had paſſed through them; therefore he formed this opinion of charcoal, being formed of the light inflammable air and azote: indeed, under the errors of the French theory he could form no other concluſion. But hear what Dr. William Henry ſays in the Phil. Trans.

Dr. Henry ſays that, Dr. Auſtin had not examined the reſiduums with ſufficient attention after firing this heavy electrified inflammable air

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" it lived in it till it was thoroughly phlogiſticated. This may
" deſerve a farther inveſtigation. I ſhould have put other mice
" into what remained of the dephlogiſticated air."

with

with pure air :—" For in one caſe the remaining " gas was inflammable, and in another that it ſup- " ported combuſtion *like vital air.*" Certainly then this laſt reſiduum had all its inflammable and charcoal conſumed in the combuſtion; therefore muſt have been fired with its full proportion of pure air, and, in conſequence, if his theory had been true, would have formed the ſame quantity of fixed air. But our aerial chemiſts have formed the moſt erroneous ideas of all their experiments. Dr. Prieſtley only obſerved this fact very lately, though if he had attended to my publications he might have obſerved it long ago. He ſays, in his experiments on the generation of air from water,

" The reaſon why, in my former experiments, I " always procured more or leſs acid, muſt have been " that, without any intention, or ſuſpecting that " any thing depended upon it, I muſt have had " ſome ſurplus of dephlogiſticated air. M. Lavoi- " sier I alſo perceive to have taken it for granted, " as I did; that after either of our proceſſes, any " ſurplus of either of the two kinds of air would " only have remained unſaturated, and have been " found in the reſiduum.

Therefore by Dr. Henry's employing a greater proportion of pure air than Dr. Auſtin did, no doubt he might produce more fixed air than the Dr. as the greater quantity of fire ſet looſe by the heavy inflammable air being electrified, would, in conſequence, ſet looſe a greater quantity of the fire which neutralized the fixed air of the pure air; but as there was a greater proportion than the inflammable air required for its combuſtion; its combuſtion would not be ſo complete as to form the whole of its pure air's acid into its condenſed ſtate, but leave part of it in the ſtate of fixed air.

And when Dr. Prieſtley and I uſed too little pure air, we found that the condenſed acid united to the unburnt inflammable air, and formed with it azote; but even in firing the light inflammable air from metals with too great a proportion of pure air, they ſometimes have found a proportion of fixed air in the reſiduum.

But this heavy inflammable air though electrified, and in conſequence poſſeſſing a greater proportion of fire, is, nevertheleſs, far from the high ſtate of concentrated fire that light inflamable air poſſeſſes, therefore, will ſtill leave a part of the pure air's acid in its aerial ſtate as fixed air.

But let Dr. Henry examine the reſiduum in Dr. Auſtin's experiments, and he will not find any carbone in them, and probably ſome condenſed acid; and, in conſequence, leſs fixed air.

Indeed it clearly appears that our aerial chemiſts will always be in the dark in all their experiments and explanations till they adopt my theory, and then nothing can be more eaſy and rational.

Dr. Auſtin's ſuppoſition that *charcoal, that wonderful French element*, is a compound body formed of light inflammable air and azote, throws ſuch a ridicule upon their theory, that, I wonder they had not directed their ſhafts at it before.

Now, Dr. Henry, from what I have ſaid, the reader muſt be maſter of your explanations, Dr. Auſtin's and mine: and let us ſee which is the true one. We all agree that, there is a greater quantity of light inflammable air in the heavy inflammable air, after its being electrified: then, what produces it? is the queſtion. I ſuppoſe that the heavy inflammable airs are formed of concentrated fire; but, of a leſs proportion of fire than the light inflammable airs, and by the electric fire they receive a greater proportion. For, do not they reduce metals in a greater proportion, giving them a greater quantity of its fire? For, as Dr. Auſtin ſays,—P. 402. "When the electrified air was fired with "oxygenous gas, it was found that more oxygen "was required for its ſaturation than before the "action of the electric fluid; which proves that, "by this proceſs, an actual addition was made of "combuſtible matter."

† I will pay Dr. Henry the compliment of having clearly aſcertained that water is neceſſary to the formation of this light inflammable air in this experiment, p. 408 "If the dilatation of the carbonated "hydrogenous gas aroſe from the decompoſition "of water, the effect ſhould ceaſe when this fluid "is previouſly abſtracted. To aſcertain whether "this conſequence would really follow, I expoſed "a portion of the gas, for ſeveral days before "electrization, to dry cauſtic alkali. On attempting its expanſion, I found that it could not "be carried beyond one-ſixth the original bulk of "the gas. By 160 very ſtrong exploſions it attained this ſmall degree of dilatation, but 80 "more produced not the leaſt effect; though the "former number would have been amply ſufficient to have dilated the gas, in its ordinary "ſtate, to more than twice its original volume.—"A drop or two of water being admitted to this "portion of gas, the expanſion went on as uſual; "and I may here obſerve, that when a little water "gained admiſſion into the tube along with the "gas, in any experiment, which often happened

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† What can be more rational than this idea, that a heavy inflammable air may be expanded by imbibing fire? Do not we ſee it in all bodies? If water is formed into a heavy vapour, by expanding, it will become a lighter body; only in the former caſe the heat becomes an integral part of the body.

" before I had acquired ſufficient expertneſs in " transferring the air from water to mercury, the " dilatation went on with remarkable rapidity"

But then if this light inflammable air was owing to the water being decompoſed, certainly we ſhould have either found the pure air entire, or have found with the ſuppoſed charcoal of the inflammable air, fixed air, after the electrization. But let us quote what Mr. Henry ſays upon this ſubject: " The change in the lime water was very " trifling; but my friend Mr. Rupp, who witneſ" ſed this as well as ſeveral of the other experi" ments, and who is much converſant in the ob" ſervation of chemical facts, was ſatisfied that, " after a while, he ſaw ſmall flocculi of a precipi" tate on the ſurface of the mercury."

And will not lime water abſorb inflammable air? Mr. Cavendiſh, I here ſeriouſly call upon you, Sir; can you ſuppoſe there was any fixed air formed in this experiment. Dr. Henry ſays, " But Dr. Auſtin did not obſerve that any preci" pitation was occaſioned in lime water by agita" ting it with the electrified gas."

And Dr. Henry ſays, in his experiments, it was very trifling; but his friend Mr. Rupp thought he obſerved ſome ſmall flocculi of a precipitate on the ſurface of the mercury. Good heavens! are ſuch vague opinions to be brought in ſupport of an hypotheſis! Dr. Henry does not hazard his opinion, but he brings his friend Mr. Rupp. The precipitation of lime water is a clear diſtinct proceſs, ſo obvious to every one, that it is impoſſible to miſtake; particularly ſo large a quantity of fixed air as muſt be generated in this proceſs, according to their theory. As the light inflammable air generated was ſo conſiderable; therefore the fixed air, according to this theory, muſt have alſo been conſiderable.—

Inflammable bodies will precipitate lime from lime water, as alkohol; but this precipitation is as lime, and not as calcareous earth, this might account for Mr. Rupp's *ſuppoſed* flocculi; or alſo the acid generated in the proceſs, might unite with the calcareous earth, and account for the phenomena. But ſince our *aerial philoſophers* are ſo ardently and enthuſiaſtically led away by the French *mania*, let us ſee if inflammable air is not ſometimes generated where there *is no charcoal*. M. Monge found that the electric ſpark would turn fixed air into inflammable air; and this, according to their theory, he ſuppoſes from a decompoſition of water: but, as there was no pure air generated the other ſuppoſed conſtituent of water, he ſuppoſes it muſt have combined with the mercury.—But Mr. Monge unfortunately had not ſuch a friend as Mr. Rupp, with his keen viſion to diſcover that the mercury was calcined in the operation, which we know it muſt have been if the mercury had imbibed it. Yet, true it is, the ſame phenomena will take place if the fixed air is electrified over water.§

Now, Sir, attend to my explanation: this electric fire will give the calces of metals ſuch a quantity of fire as to reduce them; if paſſed through the phoſphoric acid, will, from the ſame cauſe, form it into inflammable air, (and alſo common fire will do the ſame with the phoſphoric and the calx of lead) and, if paſſed through the nitrous acid will give it ſuch a quantity of fire as to form it into an air that will allow bodies to burn in it with a moſt brilliant and enlarged flame.—Alſo, if paſſed through the cauſtic volatile alkali, will give it ſuch a quantity of fire as to form it into inflammable air. Likewiſe fixed air is formed into inflammable air by fire; for, if pure limeſtone is expoſed to a ſtrong heat, it will diſcharge inflammable air, owing to calcareous earth having ſo ſtrong an attraction for its air, that it will not part with it till it has got ſo ſtrong an impregnation of fire as to form inflammable air.* But if fixed air is united to the earth of mercury, as when a ſolution of mercury in the acids is precipitated by the fixed alkali, the acid unites to the alkaline ſalt, and the fixed air to the mercury.|| See Dr. Prieſtley, Vol. 6, Sect. vii. p. 162. [This

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§ Beſides as the electric fire only paſſed from one conductor to the other: and only paſſing through the fixed air would not act upon the mercury: alſo how could they ſuppoſe if pure air had been ſet looſe in the proceſs it could act on the mercury? But all theſe reflections are foreign to our *aerial* philoſophers; their reaſonings, according to a perſon in the Gentleman's Magazine, "it muſt and does."

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* It cannot be from decompounding water, for here is no metal to attract the other component part, pure air.

|| In my Letter, page 86.—' The phoſphoric acid is obtained from burning phoſphorus, and was one leading experiment which made Mr. Lavoiſier adopt his theory of the compoſition of acids, from ſeeing a quantity of dephlogiſticated air imbibed.—But as we obſerved before, unfortunately for this theory, which of itſelf directly overturns it, the phoſphoric acid when united with the calx of lead, only produces inflammable air, and if the proceſs is puſhed by a ſtrong fire, the calx will be reduced.—Here then the phoſphoric acid (agreeably to their mode of reaſoning) is formed of phlogiſton only. What then becomes of the dephlogiſticated air which was imbibed in the proceſs of burning the phoſphorus? but the vitriolic acid may be made into phoſphorus, with the aid of animal bones and inflammable air. In ſhort, in the courſe of this Letter, it will be found that there is but one general acid in nature, and that they may be formed into one another.

' The vegetable acid, which when highly concentrated with phlogiſton will form ſulphur (ſee Prieſtley) and when fermented in vegetables, produces fixed air; with calcareous earth, produces only phlogiſticated air, this acid then agreeably to their reaſoning, is formed of phlogiſticated air.

But

This calx of mercury, by heat, will part with its fixed air as pure air; and that there is a near connection between pure and inflammable air is clear; for Dr. Prieſtley, in the ſame proceſs, ſometimes formed his nitrous dephlogiſticated air (as he calls it) into an air that would admit of bodies burning in it with an enlarged flame, and ſometimes ſo enlarged as to burn all at once, the ſame as inflammable air.

Now if this air is attended to it will clearly expound the whole myſtery; if the acid vapour is ſlightly mixed with phlogiſton, or has got a ſlight impregnation,

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'But the moſt ſtriking, is the acid called fixed air. Calcareous earths are well known to chemiſts to conſiſt of pure fixed air, and the pure calcareous earth; if expoſed to fire they will produce different airs, viz. fixed air, phlogiſticated air, common air, dephlogiſticated air, and inflammable air: juſt agreeably to this regular gradation, proving that the fire combines with the acid and earth, firſt expelling the fixed air that is looſely combined with the earth; then the acid that has got a ſmall ſaturation of fire, and as the fire increaſes, forming a regular gradation, ſo as at laſt to form inflammable air. But agreeably to their explanation, the fixed air is formed of all theſe airs. That the calcareous earth is not impregnated with phlogiſton, is proved from its forming pure dephlogiſticated air with the nitrous acid, and from its being capable of being all formed into phlogiſticated air with the vegetable acid: here, agreeably to the new aerial proofs, it cannot certainly be ſuppoſed to be all pure phlogiſton.—Therefore it ſurely cannot admit of any other explanation, than the one which my theory gives it.

'But even fixed air may be formed into pure dephlogiſticated air, if united with the calx of mercury (this calx we have all along proved to have the greateſt attraction for concentrated fire) viz. in the experiment of the corroſive ſublimate being decompounded by an alkali. If expoſed to fire, it will yield empyreal air and no charcoal; but it cannot bear the explanation that Mr. Kirwan gives of it, viz. the fixed air being decompounded, for if the fire is not puſhed, you will get the dephlogiſticated air from it, and the calx will not be reduced. Nay, to ſhew that the calx is not phlogiſticated after this proceſs, agreeably to Dr. Prieſtley's impregnation, it will only form nitrous air; but if this nitrous air is expoſed to iron it will corrode the iron, taking its phlogiſton from it, ſo as to form the air that will allow a candle to burn in it with an enlarged flame.‡ But ſometimes, in the ſame proceſs (ſee Dr. Prieſtley) it got ſuch a conſiderable proportion of phlogiſton or fixed fire as to explode all at once. And that theſe phenomena, according to my theory, are clear; for, attending to the different impregnations, it will burn differently; ſometimes the flame of the candle will be ſmaller, ſometimes larger: that is, ſuppoſe a candle' whoſe flame in burning in atmoſpherical air ſhould be the ſize of half an inch; in the nitrous air (we will ſuppoſe in ten inches of this air) its flame will ſometimes extend to one inch of the air, ſometimes to two inches of the air, and ſo on, and ſometimes it will extend to the whole ten inches; it burning the air all at once.¶ And that it is not the ſame inflammable air which comes from metals, is eaſily aſcertained by putting

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Prieſtley's own teſt, it will form with the nitrous acid, pure dephlogiſticated air again; or if united with the marine acid, and then precipitated again with the alkali, it will form again pure dephlogiſticated air. My theory here appears ſo rational, that when this dephlogiſticated air is burned, it will be decompounded: that is, its fire will be ſeparated from the fixed air, both being produced in the proceſs of burning.'

‡ There appears a great abſurdity in their theory. The nitrous air, by corroding iron and forming it into a calx, two thirds of the air is turned into that ſtate as to admit a candle to burn in it as vigorouſly as the pureſt air, or what they call oxygen-gas: but it ought to have been reverſed; as the iron is calcined all the oxygen-gas ought to have gone to the formation of the calx.

¶ If this nitrous air ſtands long enough over the iron; the calx of iron will attract its phlogiſton, forming the volatile alkali, and the air will be left with ſo ſmall a ſaturation as to form a kind of azote.

it to the different teſts; but it is preciſely an air formed of the nitrous acid, with a ſtrong impregnation of phlogiſton or fixed fire. See my thoughts on air.

Now, Mr. Cavendiſh, you muſt know that in 1781, in my firſt publication, I clearly proved that water was a neceſſary ingredient in all airs, forming a kind of baſe to them, therefore, I will appeal to every candid and unprejudiced man: if, in this experiment of Dr. Henry's, where water was neceſſary to the forming of the heavy inflammable air into the light inflammable air by electricity (and, as I have ſhewn, even heat itſelf will form the heavy inflammable airs into the light;) if it does not clearly prove that the water enters into the new formed light inflammable air, pure, and undecompounded; for if it was decompounded there muſt have been either pure air or fixed air found in it; and that there was neither is *perfectly clear*, from Dr. Auſtin's experiments, and *even* Dr. Henry's; and as heat will alſo form the heavy inflammable airs into light inflammable airs, therefore, they themſelves, *if they have any candour*, muſt admit that heat will not decompound water.§

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§ In Dr. Bewley's Treatiſe on air, p. 165.—"Inflammable airs burn in dephlogiſticated airs with an intenſeneſs proportioned to their purity, or to the quantity of their concentrated fire; and they will accordingly decompound dephlogiſticated air. If they poſſeſs a great quantity of fire, as the inflammable air from metals, they will entirely decompound it, and form the nitrous acid and water; but if they do not poſſeſs ſo much fire, part of the dephlogiſticated air will be left in the ſtate of phlogiſticated and fixed airs. I have found in ſome experiments a great quantity of fixed air formed; but by throwing into thoſe inflammable airs, which are formed by paſſing through hot earthen tubes, a greater quantity of fire, either by

Truth, honor and juſtice, will call out ſhame! if this combination is not given up. For, I do not believe there ever was ſo illiberal and extenſive an one formed. As to that part of the paper which treats of the formation of the volatile alkali; I have, in my writings, clearly proved that alkalis are formed of fixed fire, therefore it will clearly elucidate all that part of the phenomena.

I often obſerve in papers, that they are finiſhed with a ſhort note implying ſomething very deciſive in favour of their theory. See Dr. Henry's,†

Now, Dr. Henry, by diſſolving phoſphorus in any gas, and paſſing the electric ſpark through it, I have turned the phoſphorus into the phoſphoric acid, even in azote gas, alſo in the pure vapour of water, and no inflammable air was generated.

“ by heat, or the electric ſpark, and by enlarging the ſurface of
“ the tube through which the vapour had to paſs; all the in-
“ flammable airs, by having more concentrated heat thrown into
“ them, will act accordingly upon the dephlogiſticated air.—
“ And alſo, by a proper adjuſtment of the proportion of the
“ inflammable airs to the dephlogiſticated airs, I could accurately
“ aſcertain *a priori*, what would be the reſiduum; and clearly
“ deduce this concluſion, that inflammable airs are concentrated
“ fire; and that they (conformable to Dr. Harrington's hypo-
“ theſis) will act upon the dephlogiſticated airs, in proportion
“ to the quantity of fire ſet looſe. In the pure inflammable
“ airs from metals, the fire is ſeparated all at once, ſo as to
“ produce an exploſion; but in the heavier airs they burn more
“ gradually: However, if a greater quantity of fire is thrown
“ into them, they will likewiſe explode.”

† He ſays.—“ Since this paper was written I have extended
“ the inquiry to phoſphorated hydrogenous gas, which expands
“ equally with carbonated hydrogen; loſes its property of inflaming
“ when brought into contact with oxygenous gas; and affords
“ evident traces of a production of phoſphorous or phoſphoric
“ acid.”

It appears very extraordinary that Dr. Henry ſhould accuſe Dr. Auſtin of employing too little dephlogiſticated air in his experiments; when, in reality, moſt of them were performed with a greater proportion of this air, than was ſufficient to ſaturate the inflammable airs: for, a great quantity of pure air was in the reſiduum. See experiments ſeventh, eighth, ninth, and particularly the tenth; therefore he very juſtly concludes with ſaying, "It is evident that the quantity of "dephlogiſticated air uſed in this experiment is "more than ſufficient to combine with this in-"flammable air". Indeed nothing can be clearer than his experiments, and they were made upon a more accurate and juſt plan than Dr. Henry's.—Dr. Henry does not anſwer the different experiments and arguments of Dr. Auſtin, but omits the greateſt part of them. Dr. Auſtin ſays, vol. 80, p. 70. "I have already taken notice of the for-"mation of fixed air from nitrous ammoniac, "which is now well known to contain nothing, "but the phlogiſticated, light inflammable, and "dephlogiſticated airs. This ſalt, heated in cloſe "veſſels, yields dephlogiſticated nitrous air in "great abundance, mixed with a ſmall propor-"tion of fixed air. I have often repeated this "experiment with nitrous ammoniac, which in-"dicated no trace of fixed air either with lime "water, or with acids, before its decompoſition; "but, when the ſalt was decompoſed by heat, I "always found lime water rendered turbid by the "generated air; and, upon adding an acid to the "turbid lime water, have obſerved air bubbles to "be produced in it."

Now, Sir, how can you explain this? which can only be done in this way; the different acids are all formed from one: therefore they may be all formed into one another.

But to prove, beyond all controverſy, my theory. If the *pure volatile alkali* is thrown into a red hot crucible, previouſly filled with the pure dephlogiſticated or atmoſpherical air, the volatile alkali will burn; but, inſtead of turning the dephlogiſticated air into water, it turns it into *fixed air*. This experiment is beſt performed with a crucible in the form of a bottle, with a bladder fixed to its mouth containing the volatle alkali; and when the crucible gets red hot at the bottom, the alkali is then to be put into the crucible; or it may be performed without the bladder.

Now, Mr. Cavendiſh, I appeal to common ſenſe, that the ſolution of theſe phenomena can only be from my theory; and bodies burning in dephlogiſticated air, change it either into a condenſed acid and water, or the aerial acid and water, according to the degree of its combuſtion. If very intenſe, it takes from the pure air totally its aerial form, and when not ſo intenſe, leaves it in its aerial form as fixed air: in proof of which I have all along brought the moſt concluſive experiments and arguments, in my publications.—I ſhall here name only one, as I am tired with going over ſo repeatedly the ſame ground. If the red precipitate, which yields only the pureſt oxygen-gas, be heated with the filings of iron, which yields the pureſt inflammableair; the pureſt fixed air is produced, and in the greateſt quantity. The cauſe of which is this: theſe airs are generated very ſlowly, therefore the combuſtion of the airs takes place in a gentle manner, and in conſequence, the heat is not ſufficient to make that active combuſtion ſo as to condenſe the dephlogiſticated air's acid, but leaves it in its aerial form. O! when will men's candour open their eyes?

And in theſe experiments of Dr. Auſtin, no light inflammable air (the ſame as the inflammable air from iron) was generated; but only the heavy inflammable air got ſuch a great addition of fire, by the electric ſpark, ſo as to expand it to twice its bulk, and in conſequence of its containing more fire or combuſtible matter, it required more pure air for its combuſtion.

I muſt again repeat Dr. Henry's mentioning, in that vague way, the lime water being affected; for, in this very experiment, he ſuppoſes 44 meaſures of fixed air being abſorbed by the lime water. Would it not then have *ſhewn* evident and *clear* ſigns of precipitation, without the aid of Mr. Rupp? But this abſorption was the inflammable air.

Obſervations on DR. WELLS's *Paper, with other Remarks.*

Now, Sir, let us conſider another curious paper upon the colour of the blood and ſee if it will ſhew any more ſagacity, Dr. Wells's obſervations and experiments betray the ſame puerile defence of the French ſyſtem. He begins with ſaying; "Dr. "Prieſtley is, I believe, the only perſon who has "hitherto attempted to ſhew by what means "common air brightens the colour of the blood "which has been for ſome time expoſed to it."

What muſt I ſay to ſuch declarations? if Dr. Wells had read the Gentleman's Magazine, or my other publications, he might have ſaved himſelf the trouble of this performance; he then would ſee the futility of his arguments. For, I have, I hope, ſatisfactorily proved to every man who is open to candour, that the blood receives the fixed fire of the air, and from thence it receives

ceives its red colour: he will then ſee the ſubject fully inveſtigated, and, I flatter myſelf, very differently handled from his vague ſuppoſitions, even though he is patronized by you, Sir.‖

‖ The Dr. may triumphantly aſk, can he be ſuppoſed to look for Philoſophical papers, upon ſuch intereſting ſubjects, in the Gentleman's Magazine, when there is a royal ſociety inſtituted for ſuch papers? what will thoſe men ſay to this, who have hindered my papers from being received there? But I muſt here introduce a character drawn by that able ſatiriſt in his Purſuits of Literature,—Part II. p. 19. “I termed the Commentators on Shakeſpeare out of mere pleaſantry, “*black-letter dogs.*” But if among theſe ſcholars, or in *any other deſcription of the learned*, there ſhould be found A MAN, who, with the grace of exterior accompliſhment, or the fulſome ſemblance of it; with the gifts of fortune, and the rank of a gentleman; with a ſtrong devotion to literature without remiſſion and almoſt without example; with acuteneſs of mind and extenſive claſſical erudition, who, I ſay, ſhould ſo far forget himſelf as to practiſe arts which would diſgrace the meaneſt retainer to learning:—*If* SUCH A MAN ſhould be found, with fair profeſſions, and obliging attentions, ſimilar of friendſhip, but at the bottom, falſe, hollow, deſigning and malicious; who, jealous of every little advancement or lucky diſcovery, even of a profeſſional artiſt, ſhould ſtrive to depreſs the efforts of ſtruggling laborious merit, or to blaſt the rival ingenuity of his learned contemporaries; who ſhould inflict a wound with more than Parthian dexterity, and yet be ſtudious of frequenting the company of men of character to countenance his own; and finally, who ſhould collect and ſcatter around him the *virus lunare*, the vaporous drops that hang in any region of infection, that the objects of their influence may feel the blaſt of the enchanter, and know not whence it comes. *If*, I ſay, SUCH A MAN ſhould be found, I ſhall not name him, and it is not for him to lay bare his own conſcience by a fooliſh, appropriating indiſcretion. I have only ſketched out at preſent ſuch a character *in proſe*; and all I ſhall ſay further is, may He, *if* SUCH A MAN exiſt, ſtrive to wipe out ſuch actions by more than literary contrition, and deeply feel and know that he has lived, throughout the courſe of a life, not inconſiderable in its duration, under a fatal error, and a wretched abuſe of time, learning, talents and accompliſhments.”

The Dr. muſt not expect me to follow him cloſely through his vague ſuggeſtions, all of which are foreign to the ſubject: but I will refute him with his own words. He ſays, p. 422. "Now this "increaſed reflection of light, in the reddened "pieces, could not ariſe from any change in the "reflective power of their ſurfaces; for bodies "reflect light from their ſurfaces in proportion to "their denſity and inflammability; and neither of "theſe qualities, in the reddened pieces of craſ- "ſamentum, can be ſuppoſed to have been aug- "mented by common air, or a ſolution of a "neutral ſalt in water."

Now, Sir, in another place, in anſwer to Dr. Prieſtley, you ſay, "It may be urged, that, ſince "the neutral ſalts, and the different alkalis, when "ſaturated with fixed air, produce the ſame effect "as common air upon the colour of blood, if "common air acts by attracting phlogiſton, thoſe "other bodies muſt have a ſimilar operation.— "But ſurely it cannot be thought that the mild "volatile alkali, which has been ſuppoſed by che- "miſts to ſuperabound with phlogiſton, can yet "attract it from blood."

Then, Sir, I retort upon you your own arguments; can you ſay that the volatile alkali is not phlogiſton or *inflammable?* no, you acknowledge it, and I believe all chemiſts are *now* giving up Mr. Lavoiſier's *abſurd doctrine*, that the fire which comes from the combuſtion of inflammable bodies *all* comes from the air, *the abſurdity of which I have ſhewn in ſuch ſtrong colours*, that they are forced to relinquiſh it. See my treatiſe on air.

I have clearly ſhewn in my works, that pure air is formed of fixed fire, an acid and water. And alſo that the common alkaline ſalts are formed of fixed fire. But, if my experiments do not ſatisfy you, if you will look at the experiments of Dr. Girtanner* you will there ſee that the fixed alkalis are ſo combuſtible, or inflammable, that the experimenter ſuppoſed (according to the French

You ſee into what contradictions the experiments of our theoriſts lead them to: phoſphorus is ſaid to conſiſt of hydrogene and azote, then in burning, it imbibes a great quantity of pure air, this pure air then you ſay forms with azote, the nitrous acid, and with hydrogene water; then in burning phoſphorus the nitrous acid ought to be produced and not the phoſphoric: and in burning the fixed alkali both fixed air and the nitrous acid ought to be produced: alſo, in burning ſulphur, fixed air and water ought to be produced, and no vitriolic acid; ſo of the other bodies, for I am tired with the abſurdities. Thus the analyſis of the ſame bodies in different ways, what very different reſults they give. And in burning the pure volatile alkali in pure air; as the alkali according to you, Mr. Cavendiſh, is formed of inflammable air and azote; the pure air, inſtead of being burned into fixed air, as I have proved juſt before, it ought to have been burned to the nitrous acid and water, according to your experiments. But thoſe experiments of yours, of turning pure air and azote to the nitrous acid, by the electric fire, I have ſhewn to be erroneous, and you have never dared to contradict me.—And how comes the nitrous ammoniac to be aerilized into dephlogiſticated and fixed air? See Dr. Auſtin, juſt quoted, p. 38. How can you explain this? In ſhort, the whole French theory is ſo abſurd and ridiculous, it makes one loſe all patience in the contradicting of it. O chemiſtry; chemiſtry, how long wilt thou be ſhackled with this French aerial philoſophy.

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* Dr. Duncan, in his chemical news for the year 1796, ſays, "Dr. GIRTANNER has long employed himſelf in analyſing ſome "bodies hitherto conſidered as ſimple, and thinks he has obtain"ed the following reſults.

"1. Phoſphorus conſiſts of azote and hydrogene, like am"moniac.

"2. The fixed alkalis conſiſt of carbone and azote, ſoda con"taining more azote, potaſh more carbone.

"3. Sulphur conſiſts of carbone and hydrogene.

"4. The fluoric acid has a compound, probably triple baſe.

"5. Arſenic ſeems to conſiſt of carbone, azote and hyrogene.

"The proofs of theſe aſſertions are ſoon to be publiſhed at "length in Gren's Journal of Natural Philoſophy."

theory) that alkaline ſalts are formed of pure charcoal.

Now, Sir, you muſt know this theory ſuppoſes that the air, in reſpiration, takes an immenſe quantity of charcoal from the blood when it turns it red; then how can you reconcile this wonder of wonders, that charcoal, in the ſtate of alkaline ſalts, can make it red! And alſo, as the volatile alkali makes blood red, yet you ſay that the proceſs of the air is alſo to take inflammable air from the blood, and which, you acknowledge the volatile alkali is formed of. To what abſurdities do our French theories lead us; that by the pure air in reſpiration taking charcoal and inflammable air in ſuch quantities from the blood, the blood becomes red; and alſo, by adding theſe ſame bodies to black blood it becomes red. But you ſay that the red colour is owing to the pure air imbibed by the blood: but, Sir, are the fixed and volatle alkali pure air; no, they acknowledge they are the very bodies which the air takes from the blood in this reddening proceſs. But know this, Sir, that theſe alkaline bodies are fixed fire, and that either fixed or free fire reddens the blood.—And let us now conſider what authority the French chemiſts have for aſſerting that any pure air is imbibed by the blood; why Mr. Lavoiſier ſays the pure air of the atmoſphere diſappears, and there is leſs fixed air expired than in any other proceſs of injuring the air. But why, Sir, ought we not to ſuppoſe that part of the fixed air is imbibed by the blood from the motion of the lungs, and the quantity of moiſture in the proceſs? If a quantity of fixed air and pure air are equally agitated with the ſame quantity of moiſture and motion as in reſpiration, you will find the water imbibe the fixed air and not the pure air. This is an experiment which may be eaſily tried out of the body; and I alſo found that the condenſed water imbibed the fixed air and not the pure air. But even to allow you this opinion, muſt you not ſay, Sir, that when this pure air is imbibed into the blood it muſt be turned to fixed air and water, as it muſt there meet with the inflammable air and charcoal of the blood (according to your hypotheſis) there not being ſo much as an intervening membrane to obſtruct the union; and if it was not by attraction, how ſhould it penetrate the membrane of the lungs, and what bodies could attract it but the inflammable air and charcoal, according to your ſyſtem. But, Sir, to the teſt of experiments, I ſhall not give you my own, though I have made many; but Mr. Scheele who bears the firſt of characters, whoſe experiments are ſimple and clear; not like the pompous and complex experiments of the French, which are calculated to embarraſs and obſcure the truth. But before I give you the illuſtrious Scheele's experiments let me juſt hint to you a ſimple truth.

According to your theory the black globules have charcoal and inflammable air taken from them, and attract pure air in their ſtead, and they become larger and red.

Now, Sir, I ſhould ſuppoſe, if our *aerial* philoſophers will allow of reaſon, that the globules ſhould become very light, as they parted with theſe groſs bodies and received pure air; which not only made up the deficiency of the condenſed charcoal and inflammable air in point of bulk, but even encreaſed it, as they imbibed ſuch a quantity of pure air in its aerial ſtate, ſo as to increaſe beyond their former bulk, that the globules ſhould become very light, but they are nevertheleſs ſpecifically heavier than water, as they ſink in it; and, according to your aſſertion, which is certainly juſt, being the doctrine of the great Newton, "That bodies reflect light from their ſurfaces in "proportion to their denſity and inflammability."

Then, muſt it not be a very ſingular doctrine? for, the Dr. ſays, "This increaſed reflection muſt, "conſequently have ariſen from ſome change in "their internal parts." And this change to be from thoſe inflammable bodies *charcoal and inflammable air*, being diſcharged from the body and to have received pure air in their ſtead; which, according to you, is the aceſcent principle, and ſo far from being an inflammable body, attracts inflammable bodies as an acid attracts an alkali: and alſo from their denſity; then can you ſuppoſe the blood would become more denſe from parting with ſolid bodies, as charcoal? which vegetables, you ſay, are formed of, and inflammable air; of which, you ſay, the volatile alkali is principally formed, receiving pure air, in its aerial ſtate, in their room.

You ſee, Mr. Cavendiſh, this *French theory* which you are ſo enamoured of, when inveſtigated, into what abſurdities it leads you. But, Sir, I aſſert that your attachment to it is from being intereſted; as, *you ſay*, you are the author of the two great experiments, viz. of producing water by firing pure air and inflammable air; and by *paſſing the electric ſpark through pure air and azote, producing the nitrous acid*, which are ſuppoſed to be the pillars of the theory; but the fallacy of theſe experiments I have ſatisfactorily ſhewn. But as your friend, Dr. Pearſon, from them, gives you the name of illuſtrious, you cannot bear the idea of their fallacy being detected, in conſequence your illiberal oppoſition to my plain truths.

I muſt here make this remark, that when men are prejudiced in favour of a particular hypotheſis, the moſt glaring abſurdities are adopted, and the moſt obvious and clear facts are overlooked. Mr. Gough, in the Mancheſter Memoirs, vol. 4th, ſhews, by ſome experiments, that vegetation reduces pure air to fixed air; a fact, which, I fully proved in the year 1785. See my treatiſe on air.

The illuſtrious Scheele has given a number of clear and ſatisfactory experiments, made with ſuch beautiful ſimplicity that they cannot poſſibly be miſconſtrued, very different from the pompous apparatus of Mr. Lavoiſier; the reſult of thoſe experiments clearly eſtabliſh this important fact, that inſects and vegetation turn the whole of the pure air into the aerial acid; but that animals of a higher order turn only a part of the pure air into the aerial acid. He ſays, in his experiments on air and fire, page 150. "Whereas inſects and "vegetables ſo greatly change the air (atmoſphe-"rical) that one fourth of it becomes aerial acid, "I was therefore deſirous of knowing whether "*empyreal air is not that which is here changed into* "*aerial acid*, becauſe juſt the ſame bulk of air had "been converted into aerial acid, which empyreal "air occupied it."

He then gives a number of experiments which clearly eſtabliſhes the fact. I have made the ſame beautiful and ſimple experiments over mercury, and diverſifying them, ſo that there cannot remain the leaſt diſpute. And another experiment of this great man's, which, I think, immediately eſtabliſhes my hypotheſis, is, that the cauſe why leſs aerial acid appears in the reſpiration of the higher orders of animals, is owing to the agitation of the air with the moiſture in the lungs, abſorbing a part of the fixed air.*

He ſays, p. 152. "It is known that blood newly "taken, and expoſed to common Air, acquires on "its ſurface a fine florid red; and that the parts "below, after coming in contact with Air grow "likewiſe red: does the Air perhaps here undergo "an alteration? I filled one third of a matras with "blood recently taken from a bullock; and tied "it carefully up with a bladder, and ſhook it frequently: eight hours after this, I found in this "air neither aërial acid, nor any diminution of it "in bulk; but the flame of a candle was immediately extinguiſhed in it. This experiment "was made during winter; whence, it is clear, "that the above effects cannot be aſcribed to putrefaction; ſince the ſame blood, ſix days after, "was ſtill found to be freſh, and all putrefactions "produce aërial acid.—I was now deſirous of "knowing how empyreal air, by itſelf, would act "upon animals and vegetables.

"I put two ounces of ſaltpetre in a ſmall glaſs "retort, on burning coals, and fixed to the orifice "a large ſoftened bladder; (No. 35.) then I "left the ſaltpetre ſo long boiling, that I had "obtained in the bladder three-fourths of a quart "of empyreal air: I tied up the bladder, removed it from the retort, and fixed a tube to its "orifice; and having quite emptied my lungs "from air, I began to breath the air out of the "bladder (No. 84) This ſucceeded ſo well, that "I was able to take *forty* inhalations before it became troubleſome to me; and laſtly, exhaled all

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* That difficulty and laborious breathing of animals, when ſtruggling for life, will even act upon azote. Dr. Prieſtley's experiments ſhew, even that air in laborious breathing, is received into the ſyſtem.

" the air from my lungs into the bladder. The
" air ſeemed not much to be diminiſhed, and
" having filled a glaſs with it, a burning candle
" introduced into it continued to burn.

Now here it clearly appears that the agitation made the blood abſorb all the fixed air. But that it was owing to the agitation, I aſcertained by taking a quantity of black craſſamentum and placing it in a glaſs, in a quantity of atmoſpherical air; after they had ſtood a few hours, upon paſſing up a quantity of lime water, a fourth part of the air was abſorbed. Blood I found in this experiment did not act ſo rapidly upon pure air (oxygen-gas) as upon atmoſpherical air, according to the pure air it contains, this clearly accounts for the fact why animals cannot live in it ſo well as in atmoſpherical air; alſo, vegetables do not thrive well in this pure air.

P. 148. " I ſhut up ſome flies in a phial, into
" which I had laid a paper whereon I put ſome
" honey: ſome days after they all were dead; but
" they had not abſorbed any air; the milk of lime
" diminiſhed its bulk of about one-fourth part,
" and the reſt extinguiſhed a candle."

Therefore the illuſtrious philoſopher, after making many experiments, ſays, p. 150. " Theſe
" are remarkable circumſtances; that air is not
" conſiderably abſorbed by animals endowed with
" lungs, contains very little aërial acid, and, how-
" ever, extinguiſhes the flame of fire; whereas
" inſects and vegetables ſo greatly change the air,
" that one fourth of it becomes aërial acid."

I could wiſh to have theſe experiments of the great Scheele repeated by others, their ſimplicity and beauty are particularly intereſting. I cannot help expreſſing great diſſatisfaction at ſeeing the experiments now made, are thoſe of the French ſchool; to ſupport it they ſtrain every principle, and we have ſhewn, in this work, what weight they ought to have; I canonly ſay, *Magna eſt veritas et prevalebit.* But even the experiments made to eſtabliſh it juſt prove the contrary: to exemplify this I will curſorily obſerve ſome experiments of Mr. Gough's, upon vegetation, in the Mancheſter Memoirs, and alſo Mr. Cruikſhanks's, in Nicholſon's Journal: theſe experiments prove the ſame as Mr. Scheele's; that vegetation turns pure air into fixed air. Mr. Gough ſays,—Page 491, "It is evident from this experiment, "that the putrefactive fermentation, or an emiſ-"ſion of *gas* from their ſubſtance, deſtroys the "vegetative faculty of peas; from which we may "venture to conclude, that it has, ſooner or later "the ſame effect on all other ſeeds expoſed to its "influence."

But as if this French theory infatuated people's ſenſes; though Mr. Gough's experiments clearly ſhewed all the pure air was turned to fixed air, and that when the vegetables gave out any gas, they were, inſtead of undergoing the vegetating proceſs, actually in the putrefactive proceſs, and after that would never vegetate again. Yet, according to his French principles, he is of opinion, that, in vegetation, pure air is imbibed, and fixed air emitted, ſo as directly to correſpond; the ſame quantity of pure air abſorbed, directly is the ſame quantity emitted; juſt the ſame erroneous abſurdities that they have adopted in animal reſpiration. Mr. Cruikſhanks ſays, in Nicholſon's Journal, p. 338, "To be more certain of the nature of the "change which the pure air undergoes in this "proceſs, the experiment was repeated as follows:

"January 23. A quantity of barley, ſoaked in "water for two days, was introduced into a jar

" containing 46 meaſures of very pure oxygen
" gas, and inverted over mercury. At the end
" of three days the barley began to grow, and
" this proceſs continued for ten days, although
" very ſlowly: the column of gas remained ex-
" actly of the ſame height, ſo that it had under-
" gone no apparent diminution or increaſe:—
" the barley being withdrawn, the air in the jar
" was examined, and found to conſiſt of carbonic
" acid gas, mixed with only 1-50th of its bulk of
" oxygen gas. The barley was partly converted
" into malt, the quantity of oxygen being inſuf-
" ficient to produce this change upon the whole."

Now, can experiments be more plain, ſimple, and juſt! it is impoſſible to miſtake them.

And can it be ſuppoſed but that an inſect acts upon pure air the ſame as other animals, receiving fixed fire which ſupports its animal heat?—— Then it clearly receives no pure air, as it is all turned to fixed air. But can any one deny that inſects have all the principles of animal life, the ſame as any other animals; nay inſects have comparatively more energy or principles of life than any other animals; attend to their amazing activity and quickneſs of motion; and how wonderful that ſo ſmall an animal as the bee ſhall have the ſame temperature of animal heat as the elephant, and that ſo ſmall a body ſhall retain its heat. Their organs being every way the ſame, only the inſects breathe by their ſurface, *and therefore there is no agitation of the air for the moiſture to imbibe the fixed air.* Therefore, under this review of the circumſtances, as the only difference is in this, their not having lungs to agitate the air, we certainly have a right to aſſert that the phenomena is owing to this cauſe, and to no other.

* Now, Mr. Cavendiſh, can you, after this review of the phenomena, ſtill perſiſt in your errors? if you do, muſt not the impartial world clearly ſee that a ſelfiſh intereſt actuates you. Conſider, the longer you are in acknowledging theſe plain truths, the greater muſt be your condemnation; and that acknowledgement muſt be a public one.

But though Dr. Wells has made ſo extraordinary a declaration that, "Dr. Prieſtley is, he "believes, the only perſon who has hitherto at"tempted to ſhew by what means common air "brightens the colour of the blood."

Yet, in my firſt publication, in the year 1781, I there, I hope, clearly eſtabliſhed that the chyle, united with the animal lymph, attract the fixed fire of the air forming round them a ring of that active repulſion to the rays of light; ſo as to give them that bright ſcarlet colour, ſwimming in the ſerum with a flat ſurface, the ſame as oil in water, forming a repulſive quality: they are alſo oily, unctuous, and combuſtible like oil, the dark ſpot in their middle is the ſolid particle of lymph and chyle,

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* But as animals die in dephlogiſticated air (See Dr. Prieſtley's experiments page, 26 of this Letter)—Is it not moſt wonderful, as they ought to have lived ſo much better in this air, being nothing but pure air, it therefore ſhould have attracted ſo much more rapidly the *ſuppoſed* charcoal and inflammable air of the blood, and, in conſequence, impart its fire to the blood, and alſo have entered in a geater proportion into it. But, *every* chemiſt allows that the blood acts ſlowly upon it. Then how can it, in the name of common ſenſe, inflame the lungs. But, can any rational man let his prejudice, or theories be ever ſo great, ſuppoſe that an inflammation can be induced inſtantaneouſly upon the lungs, ſo as to produce death, as in the breathing of this air. For, poſitively death enſues before they can bring it below the ſtandard of atmoſpherical air; but if the animal is brought to breathe atmoſpherical air, the inflammation inſtantly vaniſhes, and it is perfectly well. O wonderful!!!

chyle, which being formed of an animal vegetable lymph and an acid; has a ſuperiour attraction for the pure air's fixed fire than its acid and water have. And that this fixed fire being ſo looſely attached to this animal vegetable lymph, is, by the mechanical action of the veſſels and muſcles, ſo propelled againſt each other, and againſt the ſides of the veſſels, that their looſe fire is ſet looſe as animal heat during the circulation, and they are brought to the lungs to get their fire renewed again. So, as in inflammatory complaints, part of theſe globules ſhall have loſt ſo much of their fire as to form that buffy coat upon the ſurface of the blood; alſo ſeen in the blood of pregnant women from the office of the placenta where no inflammation takes place: in which the maternal blood imparts its fixed fire to the blood of the infant.

Now, as in both theſe ſtates of the human body, the blood ſhews the ſame appearances, viz. this buffy coat; we have a right to conſtrue from the ſame cauſe, the globules having loſt their fixed fire.* But a full hiſtory of all theſe phenomena is connected with my hiſtory of animal and vegetable life; which I have not given to the public, owing to the ſhameful behaviour I have received; tho' I have had it by me for above theſe twenty years.

When we reflect upon the food of thoſe animals which live upon vegetable food, the ſheep for inſtance, the aceſcent graſs, nay, the very aceſcent turnip, which is its moſt fattening food; and ſhall not only make it ſo immenſely fat, which fat our *learned* theoriſts ſay is formed of inflammable air and carbone And all its diſcharges being not of an acid quality; but of the moſt *high* ſtate of alkaleſcency and putreſcency, abounding ſo much with the volatile alkali; nevertheleſs ſupporting ſo high a temperature of animal heat: and all this from the *aceſcent* turnip and the action of the atmoſpherical air in the lungs. And this proceſs, they ſay, is from the air receiving the blood's inflammable air (which they allow the volatile alkali is principally formed of) and its carbone, which is proved by Dr. Girtanner, alkaline ſalts are formed of; and that it gives the blood in their ſtead, pure air, which they ſay, acids are principally formed of.

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* *The fire in the one caſe producing the increaſed degree of animal heat, as fever; in the other being attracted in its fixed ſtate to the infant's blood, for its life and animal heat.*

Now, I appeal to common ſenſe under this review, if any doctrine can ſhew greater abſurdity? O ſcience ! ! ! But let us conſider theſe phenomena under my theory. The aceſcent turnip mixing with the animal lymph in the receptacle of the chyle, is thrown into the circulation and paſſes the lungs, attracting the fixed fire of the air which neutralizes the acid, and, by this conſtant application, it becomes highly alkaleſcent; forming fat, the volatile alkali, animal lymph, &c. Turning from the aceſcent vegetable, to the alkaline animal. Nay, if this alkaleſcent animal matter is debarred of its animal circulation, it will become highly putrid and alkaleſcent. But, in going through this putrid proceſs, the air imparts its fixed fire to it.—And, when the animal is dead, that is, wants its circulation to ſet this fire of the air looſe as actual heat, it therefore becomes fixed, forming the volatile alkali. I flatter myſelf that I have ſhewn the abſurdity in ſtrong colours, the ſuppoſing that all the fire in burning inflammable bodies comes from the air; but that the principal part comes from the burning body: then, as the volatle alkali is a combuſtible body, containing ſo great a quantity of inflammable air, it muſt have received it from the air. § No, they ſay the air receives it in ſuch quantities, from the blood, that if the proceſs ſtops but for five minutes, death enſues. The body, which in burning, acts the ſame upon the air as the blood in reſpira-

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§ I do not know a more deciſive fact, ſhewing, that the air gives out its fixed fire when injured, and that this fixed fire forms the alkali; then, the effect which the air has upon that animal, vegetable body, cheeſe. Examine a cheſhire or any other cheeſe, when it is new, and when it is very old, the latter ſtate being ſo immenſely ſtrong, abounding with the volatile alkali. Nay, in time, it ſhall become animate, and creep away.—And it is amazing how quick it injures the air; and this is done, they ſay, by imparting phlogiſtic bodies to the air; viz. the volatile alkali, at leaſt what they allow the volatile alkali, is principally formed of inflammable air, and alſo that phlogiſtic body carbon. I think, if our philoſophers were to live upon this phlogiſtic cheeſe for a time, or were to have their noſe held over it for ſome time, it would bring them to their ſenſes; the olfactory nerve being ſo near to their head; almoſt equal to the change from a ſound to a rotten egg, with which I was for feeding our prejudiced theoriſts, in order to bring them to their ſenſes. But I think either would do.

But, upon the other hand, it is univerſally known, that oranges when firſt pluckt from the tree are very ſour or acid.—But, by keeping, they become perfectly ſweet. Now, from whence does this proceed. The ſour orange is conſtantly acting upon the air injuring it; this is, according to the French theoriſts, by imparting charcoal and inflammable air to the air, and imbibing the aceſcent principle or pure air in their ſtead. But then the effect ought to be reverſed, the orange ſhould have been turned from ſweet to ſour; the alkaline principle being taken from it, and the aceſcent principle given to it. But very unfortunately, for them, all theſe proceſſes reverſe their theory.

Mr. Cruikſhanks ſuppoſes, that ſugar is formed principally from oxygen-gas; then how comes the bee's honey, as it receives no pure air by reſpiration? No, the ſugar comes from fixed fire, neutralizing its acid. It is from this, that it is, a vegetable of a hot climate. And he alſo ſuppoſes that ſugar contains leſs oxygen-gas than acids, but here the orange becomes from ſour to ſweet by the air, reverſing his opinion. *But they err in*

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cency and putreſcency, abounding ſo much with the volatile alkali; nevertheleſs ſupporting ſo high a temperature of animal heat: and all this from the *aceſcent* turnip and the action of the atmoſpherical air in the lungs. And this proceſs, they ſay, is from the air receiving the blood's inflammable air (which they allow the volatile alkali is principally formed of) and its carbone, which is proved by Dr. Girtanner, alkaline ſalts are formed of; and that it gives the blood in their ſtead, pure air, which they ſay, acids are principally formed of.

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Dr. Beddoes and others, ſeriouſly argue that the inflammatory hectic, in conſumption, is brought on by pure air, the aceſcent principle; therefore, they order their patients to abſtain from acids and vegetable food: and to uſe alkaleſcent animal food. Good heavens! is the French theory to teach phyſicians, in the latter end of the eighteenth century, that alkaleſcent food cools the blood, and acids or aceſcent food heat it. That is, to get drunk with vinegar, and cool themſelves with ſpirits of wine. Nay, they actually preſcribe the breathing of impure air in conſumptions, and living upon alkaleſcent food. ¶ But then as reſpiration

all their explanations; for how can the aceſcent principle be neceſſary to the forming of ſugar? ſugar which is a phlogiſtic body, and which rum is made from. And is it not that ſacharine quality in vegetables, that produces ardent ſpirits; and is not the malting of barley neceſſary to its being diſtilled into a ſpirit? But, all theſe proceſſes, according to our aerial *philoſophers, are, from the vegetable receiving pure air, the acidifying principle. No, clearly beyond all doubt, it is from the vegetable receiving the fixed fire of the air that it produces ſpirits, which are demonſtrably formed of fixed fire.*

¶ A gentleman told me that, the reaſon why our *aerial* chemiſts will not adopt my theory, is, that it developes their abſurdities in ſo glaring a light, that, they have not reſolution to ſtand it.

and the burning of inflammable bodies are the ſame proceſs, what becomes of all the actual fire ſet looſe in reſpiration? why, they ſay, it is imbibed by the blood, and there lulled into inactivity. But how comes the fine membrane of the lungs not to be ſcorched in the proceſs; as, the fire, even according to them, muſt paſs in its actual ſtate through it.—Nay, place the lungs in the actual fire which comes from the burning of eſſential oils; will it not burn them, ye profound *philoſophers?* In ſhort, black is white: you may as well ſupport the one hypotheſis as the other. *Oh, blind, prejudiced man!* All bodies which contain fixed fire are ſtimulating; and here they make the office of the lungs to be, to diſcharge phlogiſtic bodies from the blood, and receive aceſcent bodies (which are ſedative) in their ſtead. Then if the lungs' office is to diſcharge fixed fire, from whence comes animal heat. *But, as I have all along ſhewn, nothing can be ſo abſurd as the French theory, in ſuppoſing that all the fire in combuſtion comes from the pure air. I will juſt propoſe this obvious experiment to every man of common ſenſe; and I call upon you, Mr. Cavendiſh, Dr. Black, and all the abettors of the French theory, to anſwer it. Let two pieces of iron be ground together* in vacuo, *and they will produce a great quantity of heat and fire: and, if ground long enough, part of their ſurfaces will be formed into a calx in the form of a powder. This clear and obvious experiment, of itſelf, perfectly deſtroys the French ſyſtem. The experiment equally anſwers if they are ground in azote, fixed air, or any other: you chemiſts reflect upon this.*†

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† I believe there is not ſo ſhameful an inſtance in the annals of literature, as the treatment I have received; though they have pilfered my ideas and experiments, yet they never once mention my name. In the Monthly Magazine, when ſpeaking of the French

But I will mention another experiment more adapted to our preſent diſcuſſion.

Take a thin piece of red craſſamentum cut immediately of the ſurface, and then cut an equal thin piece of the black caſſamentum; after drying them well, then burn them in a certain proportion of pure air, and you will find the red craſſamentum injure the air more than the black, and, in conſequence, yield a greater proportion of fire.

Now, as the red globules had yielded all their inflammable airs and carbone to the atmoſpherical air, and the black globules none; how comes in this caſe, the red globules to be more combuſtible, and to have more of theſe bodies to give to the air? If any philoſopher will examine the red globules and the black globules with a microſcope, by the unctuous feel, by combuſtion, by the taſte, the former having a more active ſaltiſh taſte, by their repulſion when ſwimming in the ſerum; I am pretty certain he will ſay the red ones have more phlogiſton or fixed fire than the

French theory, its error in ſuppoſing all the heat in burning bodies comes from the air, gives the credit to Mr. Beddoes; the proving that the heat comes from the combuſtible body. Dr. Beddoes, I call upon you to know what part of your works ever ſuggeſted the idea: but you have carried the French theory to a more laughable inconſiſtency and extremity than any other writer. See your theory upon breathing factitious airs in conſumption, with my obſervations upon it. Now Sir, in this ſame magazine, when you thought that the French were complimenting our government, in aſſiſting your plans, of introducing the breathing the factitious airs in diſeaſes, you ſoon rectified the miſtake; therefore, honor and juſtice equally called upon you to have rectified the other miſtake. All my writings were forcible and clear, proving that the greateſt part of the fire came from the combuſtible body. See my treatiſe on air.

other.* It is juſt the ſame with oil. By expoſing the pureſt oil to the air, it injures the air the ſame as the blood. And every man knows that it becomes, in conſequence, more rancid, and more inflammable: and will any *ſerious aerial* philoſopher ſay that this is done by giving the air its inflammable bodies, carbone and inflammable air?—Then how, in the name of common ſenſe! can oil become more inflammable by having previouſly given to the air its inflammable bodies, carbone, and inflammable air, and receiving the aceſcent principle, pure air? As by its expoſure to the air it injured it; therefore, then, according to the French theory it muſt have been undergoing a partial combuſtion. But, after this expoſure, it is more inflammable; that is, it had more carbone and inflammable air to give to pure air, then it had before expoſure. Know this, and not any thing can be clearer, that, in conſequence of expoſure, it receives from the air its fixed fire; and, in conſequence, becomes more rancid and hot, and produces more fire in combuſtion.† If I have proved any thing (See my treatiſe on air) it is, that combuſtion is a proceſs which ſets looſe the fixed fire of bodies as well as the fixed fire of the pure air. That the pure air is neceſſary from having its fire more looſe, and therefore acts as the agent in ſetting looſe the combuſtible bodies' fire; and the more fire the combuſtible body has, in conſequence, it will require more air to ſet it looſe. Can any thing be more eaſy, more rational, more ſimple than this theory and theſe experiments, far from the obſcurity of the French theory, and their pompous parade of experiments. In ſhort, having the trouble ſo often to argue upon ſo obvious and clear a fact, it becomes very unpleaſant. What will future ages ſay of the candour and judgment of my opponents?

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* If we dare mention any old authorities againſt our preſent *aerial* chemiſts. The great Gaubius was of opinion that the red globules were more phlogiſtic. Indeed every phyſician muſt know, that blood which poſſeſſes the moſt of theſe red globules is the moſt inflammatory; that is, implying the moſt plethoric ſtate and the moſt liable to the inflammatory diatheſis. But all *reaſon* is now ſet aſide by our *aerial* experimenters, in ſhort, black is white. *O ye learned conjurors!*

† If oil is expoſed in cloſe veſſels, to this fire, it will become equally rancid, as it does in the open air. Pray, Mr. Cavendiſh, can it have loſt its carbone and inflammable air and got pure air in cloſe veſſels, or can it have received any thing but fire in this proceſs; can you have paſſed through life ſo many years and not know that the fat of meat becomes more taſty, ſalt and pungent by the fire? If, Mr. Cavendiſh, you are ſtill ignorant whether

Another queſtion introduces itſelf here; how comes combuſtion to take place in the lungs, under ſo weak a temperature of animal heat? They aſſign ſome ſilly reaſon; for the frivolity of our French chemiſts receives any kind of reaſoning; that inflammable air in its aerial ſtate, poſſeſſing its latent caloric, cannot unite with the pure air. But along with the carbone, it is united with the pure air, they both being in their condenſed ſtate, as oil. Will oils burn with pure air in this low temperature of animal heat? ‡ But to

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whether fire makes blood more rancid, aſk your tallow chandler when he ſcorches his fat, what is the conſequence? He will teach you a little philoſophy, at leaſt, a little common ſenſe.

‡ This idea, of bodies not uniting with one another, owing to their latent heat, is contradicted by every experiment. Will not fixed air unite with alkaline air? marine acid air, alſo with alkaline air? and pure air with nitrous air? in ſhort, all the acid airs. Why fixed air will not unite with lime without water, I proved, is owing to the fire of lime being ſo attracted to the earth

to ſhew their extreme errors; will not oils when out of the body, injure pure air: and there is no heat generated. Nay, ſo far from being a proceſs of combuſtion, the oil generates, as we before proved, a greater power of inflammability from it by poſſeſſing more fixed fire. And this oil when burned gives out a great quantity of actual fire, and injures the air. That when no combuſtion takes place, the air is injured, but there is no fire. Then as the oil becomes more combuſtible from it, or has more fire to give to the air when it is burned, does it not clearly appear, beyond all diſpute, it muſt have received this fire from the air. And to corroborate this theory; if the oil is expoſed to fire, in cloſe veſſels, it will attract it, and become rancid, the ſame as it does in expoſure to the air. Now, in this proceſs, it could neither part with inflammable air nor carbone, nor receive pure air. *But poſitively could only receive fire.*

If the red globules are burnt in atmoſpherical air, the reſiduum is a charcoal. Our wiſe theoriſts ſay, this proceſs is by giving charcoal to the air; but the reſiduum is charcoal. If black globules are expoſed to the atmoſphere, according to the French theoriſts, a ſimilar proceſs to combuſtion takes place; but, inſtead of their turning to charcoal, they form red globules, which, in conſequence, became more combuſtible than the

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earth that the water or fixed air cannot expel it ſingly. Juſt ſo the fixed fire of iron, the vitriolic acid or water cannot expel it ſingly, without their action is united, or elſe aided by fire, as ſteam or the acid made hot. Theſe, facts in this caſe, are unfortunate for our *wiſe* theoriſts, as indeed every thing militates againſt them; for fire is neceſſary to their action upon each other, inſtead of hindering their union; and, is not the fire in the ſteam or vapour latent? Then how comes it from this latent fire to act upon the iron, for the water will not without the fire?

black globules. If that important proceſs of reſpiration was a burning proceſs, would not the Greenlanders' blood, who live upon whale's oil, be formed of the ſame reſiduum, as when oil is burned out of the body, namely, pure water, and this to reſiſt this piercing cold climate. Bread is the food of man; If you burn it, it turns to a charcoal. But, wonderful, the ſame burning proceſs in the lungs turns it to the red globules.— And if the air, in the burning took from it charcoal, how came the reſiduum to be charcoal?

You ſee what a ſtriking ſet of abſurdities your theory leads you to. But, in inflammatory fevers, do we not obſerve through the whole ſyſtem, its temperature increaſed? See a fat man after a ſevere fever; all his fat, or oil, is conſumed, his red globules all broke down into a looſe watery blood, during the diſeaſe,and the blood puts on a ſtrong buffy coat. What can this be owing to? the great characteriſtick of the fever is an exceſs of animal heat, and the oil and red globules of the ſyſtem, are, in the ſame proportion, exhauſted. That theſe bodies did not diſcharge themſelves in their compound ſtate we are certain, for the animal evacuations ſhew no ſuch bodies: oil is a body which will not mix with water, therefore, it might eaſily be detected in the evacuations: alſo, the red blood from its colour.

If men, inſtead of confining themſelves to their experiments in the laboratory, had endeavoured to digeſt thoſe which they have already made, and to form ſome fixed principles to proceed upon, by enquiring whether the apparent reſult of thoſe experiments agreed with the phenomena of nature, they would not run into ſuch wild errors and inconſiſtencies. Atmoſpherical air ſupports putrefaction, part of it becoming fixed air in the proceſs, and the blood becomes alkaleſcent, or putrid. Then, you ſay, it is by attracting thoſe alkaleſcent bodies from the blood, viz. fixed and inflammable air; which your advocates ſay alkalis are formed of. So the blood becomes alkaleſcent by having its alkaleſcent bodies taken from it. Aſtoniſhing! but the wonder is not half over; for, you ſay, thoſe bodies turn the air into fixed air. Now, Mr. Henry has ſhewn, in his tranſlation of Mr. Lavoiſier's works, that fixed air reſiſts putrefaction, bodies already putrid became ſweet by it. And, as you ſay, a part of the pure air is abſorbed by the blood, being attracted thro' the membranes of the lungs, it muſt certainly there form fixed air, as there is not even an intervening membrane to obſtruct its uniting with the charcoal and inflammable air of the blood. Then how comes an animal's blood, when dead, to putrefy in the lungs? nay, let us take blood out of the lungs, it certainly ought never to putrefy, as it changes the atmoſpherical air around it into fixed air; and alſo imbibing air which it muſt turn to fixed air. But, I have not done with you yet. Sir, it is a fact which all phyſicians know, that alkalis make blood more fluid, and that acids thicken it. Now, Sir, the blood, by paſſing through the lungs, becomes more fluid; a fact equally well known to phyſicians, and this muſt be done, according to you, by attracting alkalis from it, and imparting to it acids, or the aerial acid, fixed air! What think you of your boaſted theory?

But let us analize the French theory of reſpiration, as given by Mr. Sequin. Mr. Lavoiſier, he ſays, " proved that four-fifths of the pure air breathed, is turned into fixed air." Then one-fifth was either abſorbed by the blood; or, what

Mr. Sequin thinks much more probable, had entered into the compoſition of water. Mr. Sequin adduces the experiments of M. M. Cigna, Prieſtley, and Hamilton, by whom, this latter opinion is ſupported. Here, then, they ſuppoſe no air enters the blood in its aerial ſtate. But, our philoſophers finding this opinion, in contradiction to the phenomena, they think, though they have no experiment to prove it, that the pure air is attracted by the blood. But if it is attracted by the blood, it muſt be by the inflammable air and carbone of the blood; in conſequence, they muſt either form fixed air or water. Mr. Sequin ſuppoſes the latter, for he does not hold the abſurd doctrine that the pure air and inflammable, or carbone, can join together, though a membrane intervenes, without it is done by attracting each other through it; if ſo, they muſt either form fixed air or water in or out of the blood. But ſome ſagacious theoriſts ſay, that pure air penetrates this membrane itſelf, and enters the blood as pure air, and when in the blood, does not form with it either fixed air or water.

You conjurors, will not animal membranes§ reſiſt the paſſage of air; how comes then the pure air to penetrate it, if it is not done by attraction, what bodies are there to attract it but inflammable air and carbone? Can your *extreme credulity* ſuppoſe that when the air is *in* the blood, it can reſiſt theſe attractions, when air that is out of the blood cannot, though a membrane intervenes; for ſhame with your puerile abſurdities.

Mr. Sequin ſays, that venous blood is impregnated with inflammable air, to prove which, he brings an experiment of Dr. Prieſtley's, that red blood, by expoſure to inflammable air, became black. But the Dr. confeſſes, not ſo black as when expoſed to fixed air or nitrous air. But, I will inform Mr. Sequin, black blood will turn red from imbibing inflammable air; will it not turn red alſo from being mixed with condenſed inflammable air in the ſtate of the volatile alkali, and alſo red from alkaline ſalts, which, they ſay, are carbone. That blood which is red, from attracting the fixed fire of the atmoſphere, upon attracting the high concentrated fire; as inflammable will looſe a little of its rich ſcarlet. For, as I have all along proved, that the concentration of fire in atmoſpherical air is more ſimilar to the concentration of fire in alkaline ſalts, than in phlogiſton. But here they have no experiment to prove that the blood contains carbone. And as they themſelves allow, that theſe combinations ſeem difficult; for we know that the application of a burning body is required to inflame inflammable air, and a heat of near 150 deg. of Reaumur to burn carbone in the open air: and they are obliged to ſay. "Although theſe combinations "at firſt ſeem difficult, for we know that the ap- "plication of a burning body is required to in- "flame hydrogen gas, and a heat of near 150 deg. "of Reaumur to burn carbone in open air; yet, in "the lungs, they take place readily without either "of thoſe agents; becauſe the hydrogen is not "diſengaged in the ſtate of gas, and therefore not "prevented, by its attraction for caloric, from "uniting with oxygen in the ordinary temperature, "and becauſe the carbone is held in a very divided "ſtate by the hydrogen."

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§ The membrane of the lungs is impervious to air, the ſame as the bladder.

How eaſily our *aerial* chemiſts account for every thing. Mr. Cavendiſh, I muſt here poſitively ſtop a little to tell you a ſtory. A gentleman, a French theoriſt, like yourſelf; was firing inflammable and pure airs, in cloſe veſſels. After one of his experiments he obſerved at the bottom of the veſſel, a body formed of burnt hair. He was agitated, pleaſed, and gratified, and told me ſeriouſly, that he found out the proceſs by which nature generates hair, and produced his ſpecimen deſcribing the proceſs. I ſmiled, he was angry, and ſaid it was no more improbable, or irrational, than their producing water, and began entering into a long and learned diſcuſſion; ſaying, that theſe burnings and exploſions were performed upon the ſurface and the heads of animals, and in conſequence, hair generated: but *I laughed, and granted that it was no more improbable, than their forming water,* and ſaid his reaſoning was ſo modern and aerial. He grew angry, propoſed a repetition of the experiments in my preſence. With elevated confidence, he prepared his apparatus and cloathed his head with his elaboratory wig, and looking me in the face, ſaying, now you ſhall ſee. I burſt out a laughing, immoderately; he ſtormed, I gravely took of his wig and pointed to the place where it was ſenſibly burnt. Conviction ſtruck him; he hung his head, and I left him with ſaying; ſuch are the *aerial* experiments and principles of modern chemiſts.

Then, the *aerial* reaſon why inflammable air and carbone burn in the low temperature of animal heat, is owing to theſe bodies not poſſeſſing caloric, which they do in their gas ſtate. Pray, Mr. Cavendiſh (for I have not yet done with you) charcoal has none of this gas in its ſtate of charcoal, why does it not burn in this low temperature? Oils, ether, and many other bodies may be formed into inflammable air; why do they not burn in their condenſed ſtate, in the ſame temperature?

But even the inflammable air, if expoſed to water, will be condenſed by it, and it is ſtill obſtinate in burning. Nay, the pure inflammable air, which you ſay water is formed of, will not burn when it has loſt its caloric. An experiment of the illuſtrious Scheele, *who had no littleneſs about him.*

Mr. Scheele, by placing iron in diſtilled water, obtained an inflammable air in its condenſed ſtate, which, as Mr. Scheele ſays, "it is an oily ſcum "which ſwims upon the top of the water."

Now, Mr. Cavendiſh, will this condenſed inflammable air or charcoal inflame with pure air, in this ſtate of condenſation; or low in this temperature? Does not conviction flaſh your abſurdities in the face? What a herculean labour have I, it is like cleaning the Augean ſtable!

Dr. Crawford's opinion, that, the venous blood requires more heat to raiſe its temperature than the arterial blood, I directly aſſert, is erroneous, (See my treatiſe on air) where I ſhew the errors of his experiments. I muſt again put this queſtion to every phyſician; from whence comes the heat in an inflammatory fever, which often continues ſuch a length of time, and reduces the fatteſt patient to the leaneſt? In their doctrine, the animal heat is ſaid to reſide in the venous blood, poſſeſſing a little more than the arterial, and it receives that in paſſing the lungs, and it is loſt by the blood once circulating through the animal, and comes to the lungs to receive a freſh ſupply.†

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† Dr. Crawford, from whom they take this erroneous opinion, only ſuppoſes the difference to be 11.5 to 10. But ſee what Mr. Sequin thinks. "To explain this, he has recourſe to thoſe experiments of Dr. Crawford, which prove that the capacities for containing

Now, in my theory, the globules of the blood are formed of lymph and fixed fire; and alſo the fat or oil of the animal is formed of more condenſed fixed fire. That the bear in Greenland, with this

containing caloric in arterial and venous blood, are nearly as 11.5 to 10; that is to ſay, if it require a quantity of caloric, repreſented by 11.5, to heat a pound of arterial blood from Zero to 30°, it ſhall only require a quantity as 10, to heat a pound of venous blood from Zero to 30°."

Is it poſſible this difference between the heat of the arterial and venous blood could produce the increaſed heat in an inflammatory fever. But will it account for the animal heat? Suppoſe a man's blood was in this proportion, even to be near half of his ſolids, then the blood in the circulation would be able to give to the ſyſtem ſcarce one degree of heat. But then, how comes it that, inſtead of animal heat being not one degree above the circumambient air, it ſhall often be above one hundred in froſty weather. From whence then is the ſource, as the lungs give ſo very little to the arterial blood; how muſt a fly ſupport its animal heat?

The calculation, according to Dr. Lower, that the blood in an ordinary man, is 20 lbs. therefore this blood muſt give out an immenſe quantity of heat; indeed to keep the other nineteen parts of the animal in the heat of ninety-ſix, in returning to the heat, for the blood is eſtimated to be in the proportion of one 20th of the weight of the whole, according to Dr. Moulin. Then, how is it poſſible to ſuppoſe that this ſmall quantity of heat which the arterial blood poſſeſſes above the venous (viz. 11.5 to 10.) could not only ſupply animal heat in Iceland; but, in inflammatory fevers to increaſe it to ſuch an exceſs. But let us attend to local inflammations, we will ſuppoſe an inflammation in the foot; the increaſed circulation according to them, muſt make the carbonated hydrogenous gas of the venous blood enter the arterial, and increaſe the heat from 11.5 to 10: then O wiſe philoſophers, the inflamed foot could only receive a little more than one degree of heat above the uſual temperature; after that was gone where is the reſervoir to repleniſh it again? the blood muſt go to the lungs before this heat can be renewed. Pray let me aſk them if, under this review, we could have either animal heat or general or local inflammations? Why, I will confidently ſay, that this *wonderful reſervoir* would not ſupply animal heat enough for your little finger, Mr. Cavendish.

immenſe ſtore of fixed fire, in the form of fat, ſhall be able to ſleep over the winter; but, in the ſpring his fat is gone, being decompounded, and going to the ſupport of his animal heat, which, though low in his ſleep, is ſtill conſiderably ſuperior to the atmoſphere. And another phenomenon it beautifully accounts for; if an animal body is laid in water, it will, in time, become a ſpecies of fat. The cauſe is this, not being expoſed to the air, it cannot attract as much fixed fire as to make it putrify, or alkaleſcent; and that proceſs does take place, and it gradually receives ſo much as to form a fat or oily ſubſtance, a kind of wax or ſpermaceti. Therefore, in a fever all this fixed fire of the animal is ſet looſe as actual heat; nature having ſuch a reſervoir of fixed fire, the fever will be able to ſupport itſelf for ſo long a time. To prove that this theory is true, examine a patient who has died of an inflammatory fever; if all his body is not principally exhauſted of its fixed fire: and, the bodies which contain this fire are not diſcharged in their compound ſtate, we are certain; for we ſhould eaſily detect them.

Now, Mr. Cavendiſh, all theſe facts no man can diſpute, and they are ſo obvious that no one can miſtake them. But I am not ſo ſanguine as to ſuppoſe they will be admitted. No, I know the combination that is againſt me, and that they can command the public opinion. But I know this. *Magna eſt veritas et prevalibit*; and when truth does triumph, beware of her ſentence: I ſhall certainly demand of the public that retribution be made, and ſuch behaviour ſhewn in its proper colours. Ignorance ſhall be no apology; for, it is impoſſible, after my writings, that the moſt common intellects can miſtake them. The gentlemen with their "little blue backed books, "and the gentleman with the great blue backed "book"* are, probably, indifferent to cenſure.—Indeed they are below notice, being only mere agents. But you and many others, Sir, whom I ſhall ſome day mention, I hope are not. If my doctrine had received a fair and candid inveſtigation, it would have been well. But, the low arts to ſuppreſs it, which have been employed, betray a meaneſs, which I ſuppoſed impoſſible with philoſophers. But, Sir, the day of retribution *muſt* and *ſhall* come. Now let me ſeriouſly call upon the public not to let their judgment be biaſſed, but ſeriouſly to think for themſelves. What muſt future ages ſuppoſe, when the truth was ſo clearly placed before them, that they did or would not ſee it? but were led by a ſet of deſigning men and their mercenary agents. Let reaſon aſſume her dignity, and ſhew the ſame ſpirit and ſenſe that our forefathers poſſeſſed. Are our ſtudies to inveſtigate nature, or to exalt the characters of a few individuals, whoſe conduct will one day be ſhewn in its proper colours.

I flatter myſelf, I have made it clearly appear to every impartial perſon that no oxygen-gas is received into the blood by reſpiration; but it is turned, in this proceſs, into fixed air, and that a part of the fixed air is imbibed. Then let us reaſon a little further upon it; though, I muſt own, I ought to aſk pardon of my candid intelligent reader in enlarging ſo much upon what appears ſo obvious and clear: but, I muſt apologize, having ſo artful and powerful a combination againſt me; aiming to overpower, both reaſon, common ſenſe, and juſtice.

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* Nicholſon's Chemical Journal's principal object is, to keep up the abſurdities of the French theory. One cannot help remarking its extreme partiality for this ſyſtem, and its defenders. In Count Rumford's paper, on the exploſion of gunpowder, only a part is given. But, in Dr. Pearſon's, the whole, nay, even a conſiderable part which the royal ſociety rejected giving to the public: it is a journal formed of all the trifling chemical papers that can be collected in favour of the French ſyſtem, while he is yet perfectly mute to any thing in its oppoſition.—But it is a moſt puerile performance.

The office of the lungs is, according to them, to take charcoal and inflammable air from the blood. Then let us ſee what thoſe bodies are:—by burning oil, which the Greenlanders *live* upon, the air, they ſay, performs the ſame office as it does in the lungs; taking from the oil both its charcoal and inflammable air; and leaving the reſiduum in a ſtate of *water*. But, as water is not capable of nouriſhing the body; the oil's nouriſhment muſt have been from the charcoal and inflammable air. Then dame nature acts *a moſt waſteful* and *extraordinary* part, contrary to her uſual œconomy; ſhe obliges the ſtomach and other functions of the body to digeſt ſuch a quantity of nouriſhment, that if it is not conſtantly diſcharged, even if the lungs ſtop but for five minutes, death is the conſequence; beſides conſuming unneceſſarily a quantity of food. Wonderful, O you *aerial* chemiſts! But, as they ſuppoſe, the food, by paſſing the lungs, becomes more ſtimulant, according to Dr. Goodwin.—How comes this ſtimulus to be increaſed by taking thoſe bodies from it, which ſpirits of wine are ſaid to be formed of; as the burning of ſpirits of wine: the ſame phenomena are produced, the reſiduum is water, and the air is turned to fixed air. Then, by taking thoſe ſtimulating bodies from the blood and turning it into water, the heart is ſtimulated by this water to do its office; which, if the blood had gone to it with all its charcoal and inflammable air or ſpirits, it would not have ſtimulated the heart, in conſequence the animal would have died. But, as I have forced them into this opinion, that all the fire does not come from the oxygen-gas, but that the greateſt part comes from the burning body; then, how can it be ſuppoſed that the blood, by loſing charcoal and inflammable air, bodies which are compoſed principally of fixed fire, ſhould become more ſtimulant, from that cauſe producing animal heat.

Pray, you wonderful aerial philoſophers, does not fire ſtimulate the animal body?

Mr. Cavendiſh, you and other philoſophers, are to be taught the facts reſulting from common ſenſe; aſk a child if fire will burn? yes, a burnt child dreads the fire. If you had been aware of that, you would not have perſiſted in this fooliſh doctrine, after what I had before ſaid. Then does not fixed fire ſtimulate? Aſk a child if brandy will not intoxicate? here let the child be your teacher! Then how comes the blood in the lungs, if the pure air does not act upon it, to kill in five minutes? as in this low temperature, viz. animal heat, charcoal, and inflammable air cannot act upon it, they require a red heat. The reaſon they aſſign is, that the charcoal and inflammable air do not poſſeſs caloric, therefore they will unite in this low temperature. But, you vague *theoriſts*, will charcoal burn in this low temperature of heat, or will inflammable air when condenſed? Will ſpirits of wine, the moſt combuſtible body we know of, do it? though theſe bodies poſſeſs, according to Mr. Lavoiſier, none of this caloric.—Then *how* can you ſuppoſe an animal is killed by its lungs being ſtopped. If theſe *wonderful agents*, charcoal and inflammable air, had exuded through the membrane of the lungs into the air veſſels, in an aerial ſtate, and not having air to carry them off; then upon an animal, being ſuffocated, we ſhould have found the lungs filled with theſe bodies: but, even in that caſe, why ſhould they kill, as the lungs might expire them: and, you ſay, that theſe bodies are diſcharged in their condenſed ſtate; but, in ſuffocation, are they found in the lungs, and how could they kill the animal?

The ſtimulus of ſpirits of wine muſt have come from charcoal and inflammable air; as, in burning of it the reſiduum is water; and, the air, according to them, can only take charcoal and inflammable air from it. Therefore the fire and ſtimulating quality of the ſpirits of wine, muſt come from its charcoal and inflammable air; for, when the air takes them from it, the reſiduum is water. In burning oils, the reſiduum is water; and as oil affords the greateſt nouriſhment, it muſt have been from its charcoal and inflammable air.—Therefore, the two great offices of reſpiration, according to our wiſe philoſophers, are to take from the blood both its ſtimulating and nouriſhing parts; turning it into a ſtate of water. Behold your wonderful theory diſſected. Mr. Lavoiſier ſays, that water conſiſts of hydrogen and oxygen gaſſes; and that theſe gaſſes, with the addition of charcoal, or carbone, form alkohol or ſpirits. Alſo, he ſays, it appears that one pound of olive oil, conſiſts of 12 oz. and 5 grs. of charcoal and 3 oz. 2 gros. 67 grs. of hydrogen. Therefore, clearly, beyond a doubt, theſe bodies, charcoal and inflammable air, according to their doctrine, are the ſtimulus and nouriſhment of the animal ſyſtem; and, that the office of the lungs is to take both the ſtimulus and nouriſhment from the blood, reducing it to water. Theſe, O you wiſe chemiſts, are the *clear* *and poſitive deductions from your theory, beyond all diſpute; the groſs abſurdity of which,* I will not debaſe my pen *by controverting*, It could only be in the way I have named their doctrine in the Gent. Mag. for 1792. vol. LXII. page 815.* [Now,

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* ‘ This refutation of Mr. Tenant’s hypotheſis was in the hands of Sir Joſeph Banks, together with the Reflections on Phlogiſton, or fixed Fire, which I am now publiſhing in the Gentleman’s Magazine, ſo early as the 14th of March; and, very ſoon after, they were in the hands of Mr. Cavendiſh, for the purpoſe of being read before the Royal Society; an honour which I have been diſappointed of. This is not the firſt time that I have found it neceſſary to be very careful of dates; and, I hope I ſhall be pardoned for mentioning this circumſtance here, as I wiſh it to be generally known, that I had refuted Mr. Tenant’s experimental hypotheſis a conſiderable time before it was done by a writer in the correſpondence of the Monthly Review for March laſt. The ſingular, and, I hope I may be allowed to call it, abſurd, idea, that fixed air is formed of charcoal and pure air, originates from an experiment in which it appears that, when charcoal is burnt in a given quantity of oxygen-gas, there is a greater weight of fixed air left in the reſiduum than the original weight of the oxygen-gas. But Dr. Bewley has proved that part of the fixed air comes from the charcoal, which, it is well known, poſſeſſes fixed air. But, to ſhew more forcibly the different modes of reaſoning of our preſent philoſophical chemiſts, let us examine Mr. Kirwan’s explanation of Homberg’s experiment in Mem. Par. 1703: “ If ſulphur be digeſted in oil of turpentine, and then ſlowly diſtilled for ten or twelve days, it will be converted into vitriolic acid.” Mr. Kirwan ſays, that the vitriolic acid is formed from the fixed air in the oil of turpentine being decompounded, its pure air leaving its carbone, and uniting to the ſulphur.

‘ Here, according to Mr. Kirwan, the oil contains, as a component part, ſo much fixed air as to be ſufficient to form the ſulphur into the vitriolic acid. But, wonderful to relate! if this ſame oil of turpentine is burnt in oxygen gas, the fixed air which is left is conſiderably leſs in weight than the oxygen gas; but charcoal, which they ſay poſſeſſes no fixed air, leaves the fixed air heavier than the pure air. But, as a proper explanation of the experiment, let us attend to Dr. Bewley. “ Here it appears that two high phlogiſtic bodies, by the action of heat, without either dephlogiſticated air or acids, will have their phlogiſton

Now, Sir, I publicly declare to you and to the world, there is not a more clear fact in philoſophy, than that my theory is true, in ſuppoſing that the blood receives from the air, its fixed fire, which neutralized the fixed air of the pure air, and, in conſequence

giſton turned into actual heat. The oils, and likewiſe the phlogiſton of the ſulphur, are conſumed, ſo as to form actual heat, there being a ſlow and gradual combuſtion. But, if the operator is not very cautious, he will be made ſenſible of the combuſtion by the exploding of the veſſels.

"It is ſurpriſing to ſee the moſt clear and obvious facts wreſted by their hypotheſes. Mr. Kirwan ſays, it is by the fixed air uniting with the ſulphur, the ſulphur being firſt dephlogiſticated. Now, if we allow that the oil contains fixed air, what was to decompound it, what to dephlogiſticate the ſulphur, and what became of all the phlogiſton? It ſurely muſt have acted the part of an inviſible ſpirit. But, by attending accurately to the experimeut, you may ſenſibly perceive a ſlow combuſtion, with a ſeparation of heat and light." See Bewley's Treatiſe on Air, p. 60.

'But Dr. Bewley having ſhewn the fallacy of their arguments in reſpect to the compoſition of fixed air, when ſpeaking of pure air being formed by expoſing pure water impregnated with fixed air to the rays of the ſun, concludes in this jocular ſtrain; "Now, can Mr. Lavoiſier, upon the formation of pure air from fixed air, find the carbone, which ought to have been depoſited in the water, being ſet free from its combination with fixed air? Nay, will it not give our reader a laughable ſurpriſe when I tell him, that Mr. Lavoiſier ſeriouſly propoſes a manufactory to obtain charcoal by the decompoſition of fixed air?— See his Elements, p. 230. But I will hint to him a better manufactory, and one more conformable to his hypotheſis. He ſays, that water conſiſts of hydrogen and oxygen gaſſes, and that theſe gaſſes, with the addition of carbone, or charcoal, form alkohol or ſpirits. Now, as the river SEINE produces plenty of water, and as charcoal is a cheap commodity, the tranſmutation of water into ſpirits would be a manufactory that would turn to good account. This would lower the price of French brandy in Old England; or, as ſeveral of our Engliſh chemiſts are no leſs induſtrious and ingenious than Mr. Lavoiſier, the Thames might be turned into good Britiſh ſpirits; which would render conſequence, becomes a ſtimulus to the heart, and, in ſhort, to the whole animal. From muſcular action, and the beating of the arteries, this fixed fire is decompounded into animal heat; and, in conſequence, the blood is brought again to the lungs to get it repleniſhed with fixed fire.

Now,

that article ſtill cheaper. But, alas! this, I am afraid, will ſtill be one of the chemical deſiderata; and, as this kind of chemiſtry will not effect ſo much good, an alarm may be ſpread on the other hand. For, according to them, water is formed of inflammable and oxygen gaſſes, two bodies the moſt combuſtible in nature. If, therefore they ſhould be able to ſet the Thames on fire, London would be in danger of being reduced to aſhes."

‘ Had our chemico-aërial philoſophers not ſtuck ſo cloſely to their experiments in bottles and glaſſes, crucibles and gun-barrels, but looked into nature, and tried if their theory, would account for, or be confirmed by, other phenomena. they would have foreſeen and avoided the dance of abſurdities into which it has led them.

But to follow Dr. Bewley's facetious manner, Mr. Lavoiſier, in his Elements, p. 106, ſays, It appears that one pound of olive oil conſiſts of 12 oz. 5 gros, 5 grains, of charcoal, and 3 oz. 2 gros, 67 grains, of hydrogen." Now, wood in being charred well, by the action of the fire, gives out nearly the ſame proportion of hydrogen, or inflammable air. Then wood and olive oil are the ſame body. But, to be more exact, by heating charcoal red-hot, you may make it imbibe the ſame proportion of the pureſt hydrogen from metals, about the fourth of its weight, chemically attracting it with great avidity, then they ought to form olive oil; but would Mr. Lavoiſier, or his adherents, wiſh for ſuch oil to dreſs their ſallads with? Moſt of our aliments are ſuppoſed, by theſe new theories, to be principally formed of carbone, or charcoal; but I am afraid it would be found difficult of digeſtion. Mr. Tennant, in his decompoſition of fixed air, formed charcoal that would not burn, it being principally the earth of the lime, or, in other words, charcoal upon a par with the ſallad-oil of Mr. Lavoiſier.

Yet ſuch are the abſurdities of our modern theories. But, as Mr. Kirwan, in his Eſſay upon Phlogiſton, p. 52, ſays, "It is impoſſible to deny all credit to thoſe who aſſerted that lime-water was precipitated by taking the electric ſpark in common

air,

Now, I call upon all the philoſophers, teachers of chemiſtry, &c. no longer to patronize this abſurd French theory, ſo oppoſite to common ſenſe. *But if they are ſtill deaf to reaſon, candour, and juſtice, I hope the impartial world will paſs a juſt, public, and fair cenſure upon them.* I have called upon them to relinquiſh or defend their abſurdities. I have challenged, coaxed, and irritated them to it; but all to no purpoſe. They well knowing that to defend them, would bring them to public notice; and their abſurdities would be condemned, laughed at, and ridiculed.—Therefore, they preſume, upon their ſuppoſed *great* names, their pompous apparatus, and their artful combination.

But I poſitively declare, that all their boaſted experiments in their laboratory, are only to be rationally explained upon my theory; calling upon them to produce *even* one againſt it; for, when cloſely examined by their own, there are the ſame abſurdities, inconſiſtencies, and errors, as I have juſt ſhewn that there is in its explaining the phenomena of reſpiration.

But let us now conſider the proceſs of vegetation, its action upon the air, and ſee if it will not ſhew the ſame errors in their theory. Vegetables act upon air the ſame as animals do; reducing it to fixed air. We know that vegetables cannot live without air, the ſame as animals. And, according to our wiſe theoriſts, it is by giving to

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air, though it did not ſucceed with Mr. Cavendiſh, either from his uſing an inſtrument of different power from that uſed by others, or air phlogiſticated by a different proceſs." Now, in order to conclude this long note, permit me to aſk Mr. Lavoiſier, if he can poſſibly ſuppoſe the electric ſpark to be charcoal?—Yet, according to his theory, it muſt be charcoal by turning pure air into fixed air.

the air their charcoal; and, they ſay, that charcoal is the food of vegetables, it being this body they receive from manures. And it is this body that they ſay the air receives from them, ſo, the vegetables are juſt the agent to tranſmit this body to the air, then how are they nouriſhed? I ſuppoſe they will ſay a part is retained by the vegetables. But let us ſee if that is the caſe.

If you take a muſtard ſeed and examine the charcoal it contains and then take one and place it in cotton and moiſten it with the pureſt diſtilled water, it will grow moſt luxuriantly, producing a number of the ſame ſeeds; here is a great accumulation of charcoal, then, how could it have received it? Why, our wiſe theoriſts ſay, it is done by giving charcoal to the air. It being like Fortunatus's purſe, the more was given out of it, it became the fuller.

Can we have a more convincing argument; that the air acts upon the vegetable, by giving it its fixed fire: for, how hot and ſpicy this vegetable is, and how quick it grew! Then, doubtleſs, all this hot ſpicy oil which both the vegetable and its ſeeds poſſeſs, either came from the air; or from the water, they containing a fine green oil. And, we know that in order that vegetables ſhould poſſeſs this fine green oil, they muſt be expoſed both to the air and ſun. And, certainly, beyond a doubt, receiving their fire which the vegetable conſolidates into its ſubſtance. For, it is well known that, that part of the vegetable which is covered or excluded from the air and ſun, becomes white and blanched, looſing its fine green oil which gives it that green colour. Therefore, demonſtrably as this green oil is got from the air and ſun, it muſt be formed of fixed fire, and not of carbone or inflammable air; as the air could not give the vegetable theſe bodies; nay, if it is not, they ſay, conſtantly taking theſe bodies from it, the vegetable dies; and the ſun could not give it charcoal, &c. Then does not this immediately prove, that this oil conſiſts of fixed fire, and when it is burnt in the open air, the air's fixed fire being ſet looſe, ſets alſo looſe the oil's fixed fire? In conſequence, the air is left as aerial acid, having loſt the fixed fire in both proceſſes, viz. vegetation and combuſtion.

And another obſervation I muſt make upon this vegetable: as it has nothing but the diſtilled water, no earthly mineral ſubſtance to enter its circulation, it thereſore receives all its nouriſhment from the air; and which, we have ſhewn is principally fire, either fixed or free: therefore, theſe circumſtances ſufficiently account for its hot pungent taſte and quality, and its growing ſo quickly. Why all the hot aromatics grow in hot climates, is owing to fire abounding ſo much there; while the more aceſcent vegetables grow in colder climates.—Let the world be no longer impoſed upon by a ſet of deſigning men; a ſet of ſuperficial obſervers, experimenters, and reaſoners, but think for themſelves.

It is very extraordinary how the philoſophical world is blinded by the authorities of thoſe to whom it is attached. Dr. Prieſtley had fallen into the error that azote was the proper air for vegetable life; but, which opinion was clearly confuted, both by the great Scheele and myſelf. Yet Mr. Gough pertinaciouſly adheres to it; apparently becauſe Dr. Prieſtley was of that opinion. Yet, even his own experiments directly confute it. He ſays, p. 493. "Hence it appears, that the "putrefactive fermentation deſtroys the vegeta- "tive power of ſeeds ſurrounded by *azote* or co-

"vered by water: conſequently the preſence of "*oxygene* is neceſſary for preventing this deſtructive proceſs; which it does by producing "another, that may be called the *vegetative* fer"mentation."

But then, according to that blind partiality, he ſuppoſes that vegetation, after the firſt period, differs as the vegetable gets older; and, the experiment to prove this, is, a ſlip of ſpearmint, the bottom of it was placed in river water, with all its pure air, and alſo expoſed to the air; but the top was placed in azote, and expoſed to the light. He ſays, "The leaves began to wither in a few days."

My reader will ſmile, no doubt, at his proof.— The leaves falling off, "A number of freſh ſhoots "appeared in their places, both under the water "and under its ſurface." But, Mr. Gough, do you not know that water, with vegetables in it, will produce pure air, when expoſed to the light; and alſo, that water contains a great quantity of pure air? Therefore, this active vegetable will receive oxygen-gas ſufficient to make ſome freſh ſhoots. But if azote was the proper air for vegetables, how came the leaves to wither ſo immediately. If you were expoſed to an atmoſphere that made your limbs drop of, would you ſay that was the proper air for animal life; and which immediately killed infants, as it does the infant vegetable? the freſh ſhoots appearing, only proves that the ſlip had an internal circulation.

But, Sir, take two vegetables; both placed in the earth, and even in water, with one of their upper ſurfaces in atmoſpherical air, and the other in pure azote, both over mercury, and then you will clearly ſee that the one is the proper pabulum of vegetable life, and the other directly noxious to it. Vegetables require pure air, and act the ſame upon it as animals do, receiving its fixed fire. But their blood and juices have not that ſtrong attraction for fixed fire which animal blood and juices have. Diſcovered by the one becoming putrid ſo much ſooner than the other.†

The ſheep is an animal, whoſe paſturage enriches the ground conſiderably more than any other animal. Then, how is this to be accounted for by the French theory; as, according to it, the ground muſt be incorporated with more charcoal: and, as the office of the ſheep's lungs is to give the air charcoal, from whence then could the ſheep give the ground the charcoal? Alſo, it is well known that, if the graſs which the ground produces, was regularly ploughed into it again, the pooreſt ſoils would, by this management, become the richeſt.

How is this accounted for? Vegetables, you ſay, give charcoal to the air. Nay, if it is not conſtantly doing it the vegetable dies. Then, undoubtedly, the ſoil ought to have been exhauſted of its charcoal; even though the vegetables were ploughed into it again. Is it not well known that, expoſing the earth to the air, by what the farmers call fallowing, the ground becomes richer? Then this, according to your theory, muſt be by the air receiving charcoal from the earth. But attend to my explanation.

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† That charcoal contains aerial acid, as well as inflammable air, appears, from an experiment of Mr. Scheele's, p. 182.—"The charcoal is moſt proper for the purpoſe, ſince it is a ſulphur compounded of phlogiſton and aërial acid. If coals be ground together with alkali, made cauſtic by quick-lime, or fire, and then diſtilled in a glaſs retort, in an open fire. a great quantity of inflammable air is thus obtained, containing no aërial acid: if a bladder be tied to the mouth, the alkali, on the other hand, loſes its cauſticity aud effervеſces with acids."

The ſheep being the moſt enriching animal for ground, is owing to its fleecy coat in reſiſting the air's taking from it its animal heat; in conſequence this heat is retained in the body, and forms fat or oil; and its manure is, in conſequence, ſo much more phlogiſtic, or poſſeſſing more fixed fire. Alſo, as vegetables act upon the air the ſame as animals, receiving its fixed fire; and the ſun likewiſe giving them fixed fire; when they are ploughed into the earth again, it muſt receive much additional fire. Vegetables are well known to poſſeſs a degree of vegetable heat above the circumambient air: in ſhort, vegetables are only animals of much lower degree. And do we not know, that the different ſpecies of both imperceptibly run into each other; naturaliſts not being able ſometimes to diſtinguiſh the gradation.—Oils afford the greateſt nouriſhment both to animals and vegetables, and are they not formed of fixed fire? Alſo, the beſt manures are thoſe which poſſeſs the moſt fixed fire; and, likewiſe, which conſiſt of bodies that have the greateſt attraction for the air's fixed fire. In fattening animals they often give them food made aceſcent, which attracts the air's fixed fire.

Chemiſts, ſeeing the extreme abſurdity in which I have placed Mr. Lavoiſier's theory of caloric, upon Dr. Black's doctrine of latent heat; have, therefore, run into the oppoſite extreme, and ſay, all the fire comes from the burning body. They think, that they can reconcile this change with the reſt of Mr. Lavoiſier's ſyſtem: one would ſuppoſe I had not to contend with rational men, but children. They actually cannot carry their ideas to, or reaſon upon the moſt obvious and clear facts. If in burning metals, as there eſcapes ſuch an immenſe body of actual fire, which, according to this new doctrine, muſt have reſided in the metal; therefore muſt not this immenſe body of fixed fire have had ſome eſſential office to do in the metal, to change its properties, and to alter its chemical principles. Can you, Mr. Cavendiſh, reconcile their abſurdity? We ſee earths by being expoſed to a ſtrong fire, formed into glaſs: if fixed fire is uſed as alkaline ſalts, it will aid the proceſs; are we to commit a greater abſurdity by ſaying this alkaline ſalt is pure charcoal, according to ſame late experiments.

If I add alkaline ſalt to the calx of lead it will form it into a glaſs; if the volatile alkali or alkaline air, &c. it will form it into a metal; even with thoſe calces of lead which have given out all their pure air, theſe phenomena will take place.† Then by burning the metal the volatile alkali will be conſumed as actual fire, and the metal becomes a calx again. If in burning nitre and lead a great deal of actual fire is let looſe and the metal becomes a calx; here the fire, they allow, comes from the metal.‡ If I burn nitre with charcoal, all the fire muſt come from the charcoal; but if I burn the nitrous acid and eſſential oils, then all the fire muſt come from the oils; or in burning bodies in the dephlogiſticated marine acid air the ſame phenomena take place: then let us enquire how the different combuſtions are conducted. According to this new theory, the nitrous acid, and eſſential oils, muſt burn in this manner:

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† Here it clearly appears, that fire of a high concentration forms metallic earths into a metal, and fire of a leſs concentration into glaſs. Even fire will produce the ſame, if ſtrongly urged to the calx of lead (without addition) it will form the calx into a metal, but if not ſtrongly urged more of it will become glaſs.

‡ And in Mr. Cuthbertſon's experiments, by the active fire of electricity, the metal will be burned into a calx *in vacuo* in fixed air or in azote, here there was no oxygen-gas in the proceſs.

the pure air of the nitrous acid muſt attract the charcoal and inflammable air of the eſſential oils, and upon the oils looſing their carbone and inflammable air, their fire alſo becomes looſe and actual; ſo that, inſtead of inflammable bodies or phlogiſton and caloric repelling one another, according to Dr. Crawford, it is the reverſe: for, as bodies part with the one they part with the other, and as pure air imbibes the one from the burning body, the other alſo leaves it.

Poor Dr. Crawford! if thou hadſt been alive, thou wouldſt have ſeen how our *aerial* philoſophers mangle every thing, even thy erroneous doctrine; the doctrine upon which they founded their own. Eſſential oils, burning in air, leave a reſiduum of water: here then eſſential oils conſiſt of charcoal, inflammable air, and caloric. But let us take pure carbone and pure inflammable air ſeparately, and burn them; charcoal and pure air likewiſe burn: then charcoal conſiſts of carbone and caloric, inflammable air and pure air burn in the moſt violent manner, then inflammable air conſiſts of inflammable air, and an immenſe quantity of caloric; condenſed inflammable air forms an oil, then it alſo poſſeſſes an immenſe quantity of fire.

Now let us carry *this wiſe aerial* doctrine to reſpiration and putrefaction. In reſpiration, the air attracts even through the membrane of the lungs, they ſay, theſe groſs bodies, charcoal and inflammable air, from the blood, but no heat is ſet looſe; the lungs ſhewing no more heat than the reſt of the body. In putrefaction, alſo, the air imbibes from the blood or animal fleſh, carbone and inflammable air, but no fire takes place, ſeen by placing a thermometer in a cadaverous body.—But ſtill more wonderful! Mr. Cavendiſh; I will place your abſurd theories in a ſtill ſtronger light, *if* *poſſible*. The black globules of the blood, by giving to the air their carbone and inflammable air produce no fire or heat. But, if theſe globules which are now red, be burned in the air, they will give more charcoal and inflammable air to the pure air, than they would if burned as black globules; and, in the laſt proceſs give out an amazing quantity of fire; far more fire than they would have given out if burned as black globules. Then, muſt not this concluſion ſtrike every man of *common ſenſe and reaſon*, that the black globules receive caloric from the air in reſpiration and putrefaction; and, as the pure air becomes fixed air and water, in reſpiration, it muſt be from looſing its fixed fire and water which neutralized them into that fire air, called pure air.

For if pure air is made fixed air and water, by charcoal, and inflammable air, received from the blood, in reſpiration, being to its full ſaturation (for the red globules will not injure the air any more); how, in the name of common ſenſe, have the red globules more charcoal and inflammable air to give to the air when burned, than they had before they paſſed the lungs. What can be clearer: have I not inconteſtibly proved that fixed air and water, applied to the rays of the ſun, become pure air, the fire neutralizing the aerial acid? will not the volatile and fixed alkalies neutralize acids! *and have I not proved them to be fixed fire?* They ſay that the charcoal and inflammable air, which the blood gives to the air in reſpiration, are in the ſtate of condenſed hydro-carbonated air: but, will this condenſed hydro-carbonated air, burn in the temperature of animal heat? Nay, in a far lower degree of heat bodies putrefy; which is, according to them, a proceſs of combuſtion. But, had philoſophers been anxious to inveſtigate truth, they would have known that, this hydro-carbonated air is made from ether and eſſential oils, by heat.* Then will theſe oils and ether burn in pure air, in theſe temperatures?

I ſay, expreſsly, that there is not one experiment of theirs but which can be explained upon my ſyſtem, and no other; and that I have repeatedly called upon them to prove, if they can, to the contrary; they have never been able to do it, but ſkulk behind their pompous apparatus and ſuppoſed great names.

That fire entering into bodies, in large proportions, certainly muſt make a part of thoſe bodies. Thus, in ſulphur, phoſphorus, &c. For, when thoſe bodies are burned, an acid appears (even when no oxygen gas is in the proceſs) as the vitriolic, phoſphoric, acids: and, as we ſee alkaline ſalts are principally formed of fixed fire, which we *muſt* know neutralizes acids; therefore, as the acid of pure air is neutralized, no acid appearing till it has been burnt, and, in conſequence, loſt its fire, the ſame as when ſulphur was burnt; is it not clear it is from the ſame cauſe? Which is demonſtrable: as, by expoſing fixed air and water to the rays of the ſun, we can make a pure air, of the ſame kind as the pure air of atmoſpherical air; not ſo high a concentrated air as the pure air of the laboratory. Alſo, this pure air of the laboratory is made from an acid, being neutralized by fire in the proceſs, as we have explained.

Mr. Lavoiſier was under a neceſſity, from his doctrine, of calling pure air the acidifying principle: he, therefore, called it oxygen-gas. For, according to his doctrine, thoſe phlogiſtic bodies, ſulphur, phoſphorus, and charcoal, were all turned to an acid, by being compounded with it, viz. the vitriolic, phoſphoric, and carbonic acids.—But then our late philoſophers, ſeeing that in many proceſſes, where pure air is imbibed, their doctrine ſays no acid is generated: as, in the firing of pure and inflammable airs, they ſay, water and no acid is generated. But, that there is an acid generated, in this proceſs, is fully proved by my former experiments and publications; alſo, by Dr. Prieſtley's two late publications (which are principally taken from mine) and more particularly from the new experiments named in this letter. But let us conſider this doctrine of their's more cloſely, and ſee what abſurdities it leads to. Pure air, according to my doctrine, is formed of an acid, water, and a concentration of fire, ſomething ſimilar to alkaline ſalts; and when made from nitre, of its acid and alkali; but, upon heats being applied, the acid being fixed to the alkaline ſalt, is not therefore ſo ſoon aerilized, as it is when by itſelf: but, as it has a ſtrong attraction for fire, diſcovered from its being ſo eaſily evaporized, alſo from the exploſion of gunpowder, it being from its attraction for fire, therefore, flies of with it, and produces thoſe great mechanical powers ſeen in Count Rumford's experiments.—Then, as the acid, along with the water which the nitre contains, attracts a great quantity of fire, and, as the acid attracts fire, it loſes its attraction for the alkali, ſeen from heated nitre, that even the vegetable acid can ſet the nitrous acid free from the alkali; but then it is ſet free in its phlogiſticated ſtate, or a ſtate in which it poſſeſſes a quantity of fixed fire, or that actual fire which it has fixed in the proceſs. And, as the nitre is expoſed to a great heat in the proceſs; the nitrous acid, and the water, receive ſuch a quantity as to be aerilized;

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* See the late French publications where both ether and the eſſential oils were formed into their hydro-carbonated air by heat.

lized; but, their attraction for the alkali makes them receive a ſmall proportion of it, ſo as to form that neutral body called the pure air of the laboratory: but I have ſhewn it is not ſo good an air as the pure air of the atmoſphere, as that is formed from the fire, or ſolar rays of the ſun, fixed air and water; therefore, an air not of ſuch a high concentration of fixed fire, as the pure air of the laboratory; the latter not anſwering ſo well for either animal or vegetable life, but burns with cracklings as if it was full of ſome combuſtible matter, as Dr. Prieſtley has well obſerved. Fixed air and water, by being expoſed to the ſun, will form pure air; which I have proved in the moſt unexceptionable manner. See my former writings. Therefore, when this neutral air is burned with inflammable bodies, an acid muſt always appear: if the fire is very intenſe it will take from the air its aerial form, the reſiduum being water, and an acid; as in the burning of inflammable and pure airs. Or, when ſulphur or phoſphorus are burnt in pure air; as, they are formed of the vitriolic and phoſphoric acids, with a high concentration of fixed fire; the intenſity of the burning being ſo conſiderable, along with the dry acids, being robbed of their fixed fire, or phlogiſton by combuſtion, will attract, forcibly, both the fixed air and water of the pure air; otherwiſe, how ſhould theſe acids be in a fluid ſtate, after the proceſs, if they had not imbibed water from the pure air: for, even a quantity of water may be got from them after the proceſs, and yet they will be ſtill fluid. Therefore, when in the ſtate of ſulphur, or phoſphorus, no one can ſuppoſe they contain water, but, are the dry acids united to fixed fire.

Why this phlogiſtic neutral aerial body, called pure air, is neceſſary to combuſtion, is, that its fire is ſo looſe that it is eaſily ſet free as actual fire; and therefore acts as an agent in ſetting looſe the fixed fire of the ſulphur and phoſphorus. But, the atmotſphere's air's fixed fire is more looſely combined than the pure air of chemiſts; therefore phoſphorus burns in the former in a lower temperature than the latter: alſo, the former ſupports animal and vegetable life ſo much better than the latter. For, it is owing to this looſe concentration of its fire; that Dr. Prieſtley and Mr. Bergman found, even the electrical fire paſſing through it would ſet it looſe, and leave the fixed air and water it was combined with.*

Then, as we ſee clearly that pure air is formed of an acid, there muſt always be an acid in the reſiduum, when pure air is burned. And, that the acid comes from the pure air, is certain, from the experiments of Dr. Girtanner, viz. alkaline ſalts, being burnt in pure air, turn it into fixed air; and, I ſuppoſe even our *credulous* chemiſts will not ſay that the acid of fixed air could come from the alkaline ſalts. If they do, they are not deſerving of ſerious argument; I give them up to the French philoſophical mania.‡ It is juſt the ſame proceſs in reſpiration; the black globules, being formed of a lymph and a condenſed acid, have a greater attraction for the fixed fire of the atmoſpherical air than its own acid and water, and, in conſequence, they attract it: for, condenſed acids have a ſtronger attraction for fixed fire, than when in their aerial ſtate.

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* And it is from this that Mr. Cavendiſh found atmoſpherical air, neceſſary, in the proceſs of decompounding pure airs into the nitrous acid, and, not from the azote, as I have fully proved.

‡ The exception, they ſay, is in burning pure air and inflammable air; water being the reſiduum. But why this exception? is not this, being the only one, a ſtrong argument, that, an acid is in the reſiduum upon burning theſe airs.

As nitrous air is immediately decompounded by the nitrous acid. But, the black globules cannot ſo well decompound the pure air of chemiſts, its fixed fire being more cloſely attracted; and theſe black globules, then attracting this looſe fire of the atmoſphere, it gives them a kind of repulſive fire, which ſurrounds them; making them ſo active in repelling the rays of light, and alſo a high refractory power: ſo that this fire being ſo looſe that even the mechanical attrition they meet with in the circulation, ſets their fire looſe as actual. But, they ſay, this repulſive veſicle is given by the acidifying principle, pure air; and likewiſe this refractory power: but acids immediately decompound the red globules even into their ſtate of whitiſh lymph. Dr. Wells ſays, p. 423. " If a ſmall quantity of a concentrated mineral acid be applied to a piece of dark craſſamentum, the parts touched by it, will, for an inſtant, appear florid; but the ſame acids, added to a ſolution of the red matter in water, do nothing more than deſtroy its colour."

The true explanation is this; the high concentrated acid, being dropt upon the black globules, ſets all the fixed fire of the globules looſe, inſtantaneouſly; which, in becoming actual, gives that florid appearance only for a moment. Mineral acids do the ſame with eſſential oils. The nitrous acid ſets ſuch a quantity of their fire looſe as to produce ignition. Then, well may the concentrated acids ſet the looſe fire of the globules free: but if the acids are diluted with water, they produce no ſuch phenomena, ſetting the fire of the black globules ſo gently looſe, that no floridneſs appears. By violently agitating the black globules, you produce the ſame floridneſs, by ſetting looſe their fixed fire as actual. And it is eaſily ſet looſe in combuſtion, and then it acts upon the burning bodies as nature's great agent in ſetting looſe the combuſtible body's fixed fire: and, in reſpiration, putreſaction, and other proceſſes, this fixed fire is eaſily attracted from its union with the fixed air and water of the pure air.

It really makes one ſmile at the manner of arguing which our *great theoriſts* make uſe of. Dr. Wells ſeriouſly tells us that both acids and alkalis act upon the globules by increaſing their opacity, and that the latter makes blood more florid, from that cauſe. But, he ſays, that acids make blood more opaque, and yet deſtroy the colour altogether. And, there is not a clearer fact than this, that alkalis make black blood florid, and that acids make florid blood black: but wonderful that both act by increaſing the blood's opacity, he ſays; and from the opacity of the blood being increaſed, the blood becomes florid and red; indeed, Mr. Cavendiſh, you certainly have great honor in rejecting my papers and admitting theſe and others.—— Phyſicians all agree that acids coagulate or thicken blood, that alkalis make it more fluid, this has all along been their doctrine, but our preſent theoriſts overturn every thing with their *aerial* wiſdom, which is truly as light in the ſcale of reaſon as air itſelf, and your "illuſtrious" name Mr. Cavendiſh, will not, I believe, add to the weight.

This doctrine of all the fire coming from the air in combuſtion, *even* from the ſuppoſed condenſed air in their nitrous acid, is very extraordinary. But to ſhew the extreme fallacy of this doctrine, I have all along proved; when the vitriolic acid and water are applied to iron it is calcined and an immenſe quantity of inflammable air is generated. They ſay that theſe phenomena are owing to water being robbed of its ſuppoſed pure air, it being attracted by the metal forming a calx, and the inflammable air is ſet at liberty; then the water muſt ſo poſſeſs ſuch a quantity of calor as to form it into inflammable air. Nay, even the other ſuppoſed component part of water which enters the calx muſt carry an immenſe quantity of caloric along with it: for the marine acid, they ſay, from attracting this ſuppoſed pure air from the calx of lead, receives along with it ſuch a quantity of caloric, that bodies burn in this marine acid with a far greater degree of heat than in pure air in its aerial ſtate.* But in burning lead it is calcined and an immenſe quantity of fire comes from it; this fire, or caloric, they ſay, is from the air being *condenſed* in its being attracted into this calx. Nay, what an immenſe quantity of fire comes from the burning of inflammable air and pure air; but, the water, (the reſiduum) from their explanation of theſe phenomena has more fire to give back again when it is decompounded. Can you, Sir, as a philoſopher, a man of ſenſe, and a rational man, ſanction ſuch groſs abſurdities? No, I have forced the world to relinquiſh this doctrine of caloric. Then, as an immenſe quantity of fire does come from the burning of pure inflammable airs, are we not authorized to ſuppoſe, that this fire formed an eſſential part of theſe airs? That the water could not poſſeſs all this caloric, I think *we may directly aſſert*; then, it muſt have come from the metal.—That the vitriolic acid did not poſſeſs it, appears equally certain; for, upon adding the water to it, previous to its being applied to the iron, a great heat came from them;¶ and, that great heat alone will reduce either the calx of mercury or lead, is well known. I ſay, in the Gentleman's Magazine, Vol. LXII. part 2, p. 908.*

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* Alſo the *aurum flammans*, fulminates, they ſay, from the fire of the condenſed pure air in the calx, the fire of which is ſo very intenſe.

¶ As both phlogiſtions and ante phlogiſtions, built this hypotheſis

Now,

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theſis upon Black's doctrine of latent heat; they muſt both fall to the ground: for, the phlogiſtions ſay, as pure air attracts the phlogiſton, its latent heat, or caloric is repelled, and becomes actual, according to Dr. Crawford. But, if black is black, and not white, phlogiſton is not an element, according to Stahl, but fixed fire; and enters into the compoſition of bodies, forming an eſſential part of them.

* 'We find that thoſe bodies which poſſeſs a high ſaturation of fire, viz. combuſtible bodies, when they have parted with it, will attract a more moderate ſaturation: thus, for inſtance; metals, by being burned, having loſt their full ſaturation, will attract fire in a looſer ſtate, or in the ſame ſtate that lime, or the cauſtic alkaline ſalts do; for, if a metal is diſſolved in nitrous acid, the acid will firſt attract its phlogiſton, forming nitrous air. But, if this metal is precipitated by another, poſſeſſing an inferior attraction for phlogiſton, it will be precipitated with its metallic ſplendour; and, if with the cauſtic alkaline ſalt, it will be precipitated with its fixed fire.

'In burning vegetables which poſſeſs phlogiſton, they, upon parting with it, attract freſh fire in the ſtate of an alkaline ſalt. It was for this reaſon that the old chemiſts conſidered alkaline ſalts as being formed from incineration, and they procured it by burning vegetables, which vegetables they knew did not poſſeſs it before their calcination; for, inſtead of an alkaline, they are generally poſſeſſed of an acid, or at leaſt an aceſcent quality.

'As we have always ſuppoſed alkaline ſalts to be principally formed of fixed fire of a leſs concentration than what is termed phlogiſton, we ſhall give further proofs of it, in order to corroborate the theory. Metallic earths, after calcination, poſſeſs ſo much fixed fire in an alkaleſcent ſtate, and of the quality, or diſpoſition to actual fire in this ſtate, that they act as fluxes in forming glaſs; and every chemiſt knows that alkaline ſalts are the great flux made uſe of in vitryfying bodies, or turning them into glaſs. We find that inflammable air, oils, and other high phlogiſtic bodies, form metallic earths into metals; but, alkaline ſalts of a leſs ſaturation of fire, form them into glaſs. And as the ſame bodies are capable of entering into both ſtates, ſo, if oils are applied, they will form the metallic ſtate; but, if alkalis are applied, they will form the vitrified ſtate.

'And

Now, here we ſee alkalis, which, I think, I may be allowed to call fixed fire, will either form lead into a glaſs, or metal. This fixed fire, in the ſtate of iron, by having water applied to it in the form

'And, as in the metals, ſome of the metallic earths which have a ſtrong attraction for fire will be reduced by it alone, and ſo it is by the vitrifying earths; thoſe that have a ſtrong attraction for fire will be formed into glaſs by its power alone; but thoſe which are refractory will require alkaline ſalts the ſame as the metals; many requiring phlogiſton to reduce them.

'There is a near reſemblance between metals and glaſs; from a looſe earthy ſtate they both become hard and compact; and even caſt-iron, which is a ſpecies of iron imperfectly reduced, not having its full ſaturation of fire or phlogiſton, will crack and break like glaſs.

'Still further and more powerfully to corroborate my theory. If oils are applied to the calces they will recover their metallic form; but, if alkalis, they will be vitrified. And, in reducing a calx of lead by fire alone, I have formed part into lead, and part into glaſs. And of the following fact I am well convinced: in reducing metals we generally find a part refractory, which cannot be made into metal; as in lead for inſtance. That there is ſuch a conſiderable waſte in the earth of lead called litharge, is owing to its having received an alkaline ſaturation of fire, and being thereby prevented from taking the metallic ſaturation: the acid of the air, combining with the earth ſo as to aid the fire, becomes alkaleſcent. And for this reaſon it is that we find this refuſe of the metals more adapted to act as a flux. I have likewiſe found this refractory earth to go further than the other in the ſaturation of an acid; and that it is capable of precipitating the other leſs alkalized earth when diſſolved in an acid. And it is owing to the ſame cauſe that lime cannot be made to imbibe the ſame quantity of fixed air it before poſſeſſed, as part of the earth has received a certain quantity of fire producing an alkaleſcent ſtate. Lime, from the fire it poſſeſſes, being ſoluble in water, and having, like ſalts, a bitter taſte, becomes of an alkaleſcent quality.

'But I ſhould ſuppoſe that the following will be admitted as an *experimentum crucis* to prove that the earths of metals, during their reduction imbibe fixed fire in the ſtate of what has been denominated phlogiſton. If you reduce a calx with oil or charcoal, it is ſuppoſed to imbibe phlogiſton from thoſe ſubſtances;

form of ſteam, will be aerilized into fixed fire, as inflammable air; ſo, alſo, when iron is calcined with the aqueous vitriolic acid, a great heat is generated in the proceſs, which heat is neceſſary to its being formed into an air. But, if iron is applied to cold water, the inflammable air only appears on the ſurface of the water in its condenſed ſtate; being an oil, as Scheele obſerves.|| And, I muſt again repeat, that, as Dr. Black's doctrine of latent heat, which Mr. Lavoiſier had introduced into this theory, muſt be given up: this caloric muſt have been an integral part of theſe airs, and muſt have formed an eſſential part of the qualities of them. Combuſtion will take place without oxygen-gas, as I proved, from the burning of ſulphur

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ſtanees; but, to vitrify it, you add an alkaline ſalt. There is ſuppoſed to be ſomething myſterious in the part which the charcoal or oil acts in the proceſs; but in the ſecond, as every chemiſt knows, the alkali is imbibed, and enters into the glaſs.—For that the proceſs is ſimilar is evident from this, that the calx, when vitrified. parts with the acid it had received; if from the air, in the ſtate of fixed air.

'It hath been ſatisfactorily ſhewn by experiments, that oils and alkalis will become volatile. Mr. Lavoiſier found that the vegetable acid and alkali, which he calls the acetite of potaſh, will, if diſtilled, become the volatile alkali, p. 270 of his Nomenclature. The volatile alkali. having acquired a higher ſaturation of fixed fire than the fixed alkali will form metallic earth into their metallic chryſtallization, while the fixed alkali will form them into the vitrified chryſtallization.'

|| Here is a great difference between the fire which forms the phlogiſton into inflammable air, and the fixed fire which is an integral part of its compound. The oily ſcum which is found upon water, in which iron is placed, will burn equally as well as inflammable air. Nothing can be more ſimple than this experiment: if the ſteam of water paſs through charcoal, as the ſteam carries to it both water and fire, it will form the two component parts of charcoal, viz. phlogiſton and fixed air, into inflammable air, and the aerial acid. The ſimplicity of my theory immediately recommends it.

and iron: then, it is almoſt unneceſſary to ſay, that this inflammable air, the phlogiſton of Stahl, is formed of high concentrated fixed fire, with water which forms its aerial baſis. Nothing can be more obvious. But, I aſk you, how can you explain it otherwiſe? I call upon you to ſhew.

Mr. Scheele ſays, p. 166. "I have a ready method of finding out *whether the water contains empyreal air or not:* I take, for inſtance, one ounce of the water, and let fall into it about four drops of a ſolution of vitriol of iron, and add two drops of a ſolution of ſalt of tartar ſomewhat diluted with water. It yields immediately a dark green precipitate; which, in a couple of minutes changes into *yellow*, if the water contains empyreal air; but if the water be previouſly boiled and grown cold without the acceſs of free air, or, if the water be diſtilled lately, the precipitate retains its green colour and does not turn yellow, unleſs about an hour after; and never changes into yellow, if it be kept in full glaſſes ſo that the free air can have no acceſs to it."

All my quotations from Scheele are from his experiments on air and water. This is a queſtion much agitated, viz. in the calcination of iron, with an acid and water, what is it that the calx imbibes in order to its becoming a calx. I have all along ſhewn that both the vitriolic acid and water are neceſſary to the calcination, to decompound the earth of iron of its phlogiſton; the ſame as both the aerial acid and water, are required to rob the calcareous earth of its fire, when in the ſtate of lime. This elegant and ſimple experiment of Mr. Scheele's, I think, will greatly help us in the elucidation of the phenomena.— When the alkali attracted the acid of the vitriol of iron, the iron being robbed of one of its ſoluting bodies, attracts the pure air of the water, which turns it from green to yellow. But, according to the French theory, the earth of iron had attracted its full ſaturation of pure air from water. Why then does it attract the pure air of the water? And, why does the pure air of the water change its colour from a green to a yellow? How comes it that the calx of iron, when calcined in the open air, is always yellow; whilſt its other calces are green. The calx of iron made by the ſteam of water, is always heavier than the calx made by the air; and it will never become the colour of ruſty iron, if ever ſo long expoſed to the air.—From this cauſe, it is calcined by the hot water, which forms a different coloured ruſt than the air forms; and, as the calcination from the air, is from iron imbibing its acid, therefore, there is a leſs quantity of the acid than the water, required to ſaturate the calx; the former being a ſtronger body than the latter, therefore forms a lighter calx. The iron ſtill continuing green (in Mr. Scheele's experiments) the water ſhews that the calx of iron attracted the water to ſupply the place of the vitriolic acid; but, when there is pure air in the water, it attracts it in preference, and turns yellow. [This, I flatter myſelf, is incontrovertibly a rational explanation of the phenomena.] And the proportion of pure air the calx imbibes is very conſiderable. Therefore, undoubtedly, when the vitriolic acid was united to the calx; it was not fully ſaturated with pure air, as it imbibes pure air immediately as the acid leaves it: and it will not imbibe this pure air while it contains the acid. and in water which has no pure air, the calx will not imbibe pure air by decompounding the water, as it remains green; and if it did, inflammable air, according to them, would be generated, which is not the caſe.

The experiments of Lemery are well known to every philoſopher: "Having mixed 25 lbs. of ſulphur with an equal weight of iron filings, and having kneaded the mixture together by a little water, into the conſiſtence of a paſte, he put it into an iron pot, covered it with a cloth, and buried the whole a foot under ground. In eight or nine hours the earth ſwelled, grew warm, and cracked, hot ſulphureous vapours were perceived, a flame which dilated the crack was obſerved, and a ſubterraneous fire producing a volcano in minature was lighted up, the experiment has been frequently repeated; it is to be obſerved that too much or too little water will equally prevent inflammation. About twenty-five years ago, the experiment of imitating a volcano was tried at PARIS, upon a larger ſcale; whether by the directions of the late Duke of Orleans, or of the Count Laurigaes I do not recollect. The neceſſary ingredients were depoſited under a mount, raiſed for the purpoſe, in a garden. A perfect volcano was thence exhibited, to the no ſmall alarm of the neighbourhood."

Now, Mr. Cavendiſh, how can we reconcile this experiment with your extraordinary, and I have no heſitation in ſaying, abſurd doctrine: from whence proceeded all the fire? according to Mr. Lavoiſier, neither ſulphur, nor iron, poſſeſſes any fire: and, as to the water, all its fire is diſſipated when its two ſuppoſed compoſing bodies, viz. inflammable and pure airs, united. I ſay, you wiſe *aerial* philoſophers, from whence comes the fire? here is no pure air required in the experiment, the ingredients being buried many feet under ground. This I will prophecy. that future ages will wonder and ridicule the preſent age's ignorance, and credulity, in ſuch abſurdities, being ſeriouſly advanced. Muſt not every one who has an atom of ſenſe ſee, that, in this experiment, the fire came from the ſulphur and iron, the water having a great attraction for the earth of iron, the vitriolic acid of the ſulphur, and the ſulphur alſo for the earth of iron: here is ſuch a fermentation produced, as to ſet their phlogiſton, or fixed fire, looſe as free fire. This explanation is juſt, as the ſulphur is turned into the vitriolic acid, and the iron into a calx.

What a number of experiments and ſingular conſtructions our aerial chemiſts have made; the vitriolic acid and water being applied to iron, calcine it and generate inflammable air. Another ſtrong mineral acid, the nitrous, with water applied to iron, calcine it, and nitrous air is produced. In the firſt proceſs, they ſay, the water is the calcining body, in the next, they ſay, the nitrous acid. Only reflect upon ſuch a different explanation in the ſame proceſs; you credulous chemiſts! it being almoſt impoſſible to yield to ſuch abſurdities, to what does it lead you? See page 22 of this Letter.

Candles clearly elucidate this ſubject; when they are firſt made, they readily decompound the air, attracting its fixed fire, and reducing it to fixed air; which they continue to do for ſome months. After that time they no longer act upon the air, but evaporate their oil or combuſtible matter into the atmoſphere: in conſequence, it is well known that a candle will not burn well when it is firſt made, but gradually improves as long as it acts upon the air injuring it. But, after that period it loſes; and, if it is kept for a long time it will not burn at all, having loſt all its inflammable matter. Now, Mr. Cavendiſh, according to your ſyſtem it ſhould have been beſt at the firſt; as the air acting upon it, and receiving its charcoal and inflammable air, being a proceſs of combuſtion, therefore, when it came to that period when the air would no longer receive the candle's charcoal and inflammable air, it ſhould have loſt its inflammability. But it is, at this period, in its perfection, or higheſt ſtate of inflammability.—And, after that, when it has really given out its phlogiſton to the air, it injures it not; but the candle's inflaming quality is diſcharging itſelf, ſo that in time it will not burn. What will phlogiſtions ſay to this?

You ſee, Mr. Cavendiſh, that the ſtrongeſt facts may be obtained by acute obſervation in the commoneſt departments; without having recourſe to your pompous apparatus, which is only calculated to embarras truth.*

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* The Britiſh Critic, upon reviewing my Chemical Eſſays (it being the firſt time that this learned Critic had exerciſed his liberal powers upon my theory) made his remarks upon three points. The firſt learned obſervation, was, upon the catch word being wrong, in the firſt page; and. from this circumſtance, he arrogated to himſelf a degree of penetration in being able to detect the error. To ſuch trifles as theſe I fully allow his competency. But, his next remark was not ſo lucky, for he there was travelling upon unknown ground. I had ſhewn the fallacy of our aerial philoſophers, in ſuppoſing the pure air, or oxygen-gas of the laboratory, and the pure air of atmoſpherical air being the ſame. Here the Critic triumphed, directly aſſerting I was wrong, without any explanation. But denouncing, from his *little* critical chair, my condemnation.—The next was an illiberal inſinuation againſt the validity of my experiments; his verdict being pronounced without having tried them. Now, Mr. Critic, allow me to make an obſervation or two upon your ſupreme judicature. As to your firſt bright obſervation I own you were right, that the catch word was wrong. Correcting the preſs myſelf, and my mind being abſorbed

If philoſophers, without ſtuffing their laboratories with their complicated pompous apparatus, more calculated for the pageantry of a pantomime, than for true chemical diſcoveries, had attended to the facts which are conſtantly offering themſelves to our obſervation; they would have ſeen the extreme futility of their boaſted ſyſtem.—Thus the manure of a ſtable, when piled together in a great maſs, what a prodigious heat it produces. Alſo, hay, if ſtacked too green. Now the heat produced in theſe two bodies, could not come from the air: for, the larger the maſs, the more conſiderable the heat, and it is generated principally in the centre of the maſs, where air could not have acceſs to it.

Now, Sir, I have long been thinking of the body which poſſeſſes the greateſt quantity of this wonderful charcoal and inflammable air, which, according to you, both *kills*, *nouriſhes*, and *ſtimulates* the animal; and coal is the body, as the burning of an ounce of coal will injure a greater quantity

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with the profound ſubject, ſuch errors may be found; for indeed I am not anxious about them, otherwiſe I do not know what our gentlemen with their "little blue backed books" would do if it was not for theſe trifles; therefore, I think they anſwer the purpoſe of a fly cage, to attract all the noxious inſects. And every ſpacious room ſhould have ſuch a cage.—For, if I had had a few more catch words miſplaced, this *learned* fly might not have been ſo illiberal, as they would have formed the baſis of his criticiſm. But, let me give a little advice to you and the reſt of your gentry. You are not ſuppoſed to be a body of men who are at all converſant, or have a critical knowledge of thoſe very abſtruſe ſubjects: but, if you were to frequent the reſort of men of learning, you might there glean enough to make up your monthly budget; here the ignorant part of the public, who form their opinions upon your *decrees*, might form them more juſtly. But I am afraid there is too much intrigue with bookſellers and authors for you to act ſo candidly; being the mere mercenary tools of others.

quantity of pure air than the ſame weight of any other body, and you ſay the coal injures the air by giving it charcoal and inflammable air, and in pure coal very *few aſhes* remain. Then, Mr. Cavendiſh, coal ought to be the moſt nouriſhing food an animal can ſubſiſt upon; ſuperior to oil, as it injures the air in a greater proportion than it does: that is, it gives it more of theſe nouriſhing and ſtimulating bodies, viz. charcoal and inflammable air. Now, Mr. Cavendiſh, by putting your theory to the teſt, if you were to try to live upon coals. No, Sir, I ſhould be ſorry your experiments ſhould kill you, I can only ſay, "go to bed, go to bed" and dream no more of ſuch idle, ridiculous, and fooliſh abſurdities. But, awake; with your eyes open to candour, truth and juſtice.

It appears the moſt *extraordinary doctrine*, that, in reſpiration the lungs diſcharge thoſe phlogiſtic bodies, viz. inflammable air and charcoal, and receive pure air, the aceſcent principle. The food of animals, being the aceſcent vegetables, while the animal is ſo high a phlogiſtic body, and having a conſtant temperature of 96 deg. of heat; alſo, its diſcharges are ſo phlogiſtic. But let us put this doctrine to the teſt. ' Inflammable air, which every one acknowledges poſſeſſes a large quantity of phlogiſton by being expoſed to black blood, it is abſorbed by it ſo as to loſe its inflammability, and the blood turns red. Now let us examine the red blood. By expoſing it to injured air, or foul air it turns black; and Dr. Prieſtley mentions an inſtance where the air became better, and was diminiſhed after it by nitrous air. (Prieſtley, Vol. iii. p. 77.) The application of fixed air to blood equally turns it black. In ſhort all phlogiſticated bodies, oils, ſpirits, ſalts, &c. turn black blood red. All acids and other dephlogiſticated bodies, even water, turn red blood black; nay blood turns black *in vacuo* where no phlogiſton can get to it, but only has liberty to exhale its own (as related by Father Beccaria) and the change is too immediate to ſuppoſe it is from putrefaction. But as ſtrong a fact as any, is, that animal phlogiſton turns blood immediately red; this is ſeen very ſtrikingly in the animal urine when taken in its moſt rich ſtate as diſcharged from a perſon under a high fever. For when the phlogiſton is rapidly ſet looſe and diſcharged, then it will have the ſtrongeſt effect, turning the blackeſt blood immediately red. That urine is compoſed of phlogiſton no one can doubt, from its poſſeſſing ſuch a quantity of volatile ammoniac ſalts, and from its volatile pungent ſmell. The ſaliva has the ſame effect, which is another phlogiſtic animal juice.—Beſides the rays of the ſun have the ſame effect. But water has juſt the oppoſite effect All theſe facts are ſo very ſtriking they muſt make us entertain little doubt.'

But, I will finiſh with my obſervations upon the cameleon, which is *well known* to live almoſt without food; there being well atteſted facts of a ſingle fly ſubſiſting it for a month: nay, it can ſubſiſt without that fly. See its natural hiſtory.—Therefore, I ſuppoſe, Mr. Cavendiſh, you will allow theſe two things. Firſt, that it is an animal which requires very little food. Secondly, that it can have very little charcoal and inflammable air to give to the air in reſpiration, and, that it might, in conſequence, do without lungs; as, according to you, theſe *noxious, nouriſhing, and ſtimulating* bodies, viz. charcoal and inflammable air, would not *poſſibly* be in ſuch abundance as to require their diſcharge, the wonder being how the animal could ſubſiſt upon ſo little nouriſhment. But *know*, Mr. Cavendiſh, that it has a greater proportion of lungs than any other animal; it being comparatively almoſt *all cheſt;* nay, nature not ſatisfied with that, has given it a ſingular power of throwing into its cellular membrane, over all its body, a great quantity of air, which muſt be expoſed to its blood and juices, ſwelling and enlarging all its ſurface, and, as it ſubſides, or forces out the air, loaded with your charcoal and inflammable air, it takes freſh air in again.* Now you muſt ſay, Mr. Cavendiſh, this fly poſſeſt ſo much of theſe nouriſhing and ſtimulating bodies, that, if nature had not given it this ſingular and extraordinary faculty of making its body *in a manner all lungs;* it would not have been able to have diſcharged, in the courſe of a month, all the nouriſhing and ſtimulating bodies which this fly poſſeſt, beſides, the quantity which the cameleon would require for its nouriſhment and animal heat.

But, Mr. Cavendiſh, let us ſee how theſe phenomena agree with my theory. As it has little food to attract and decompound the air, therefore, that which it has to form its blood ought to be as much expoſed to the air as poſſible, in order to attract as much of the air's fire to ſupport its animal heat. And that its blood does attract a greater proportion of fire, is proved; for, its ſurface being ſo phlogiſtic, as its ſkin (another ſingularity of the animal) changes its colour into ſuch vivid and varied hues.

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* Chambers' dictionary ſays, "The trunk of the body is properly all breaſt, for the creature has no belly, its ribs being continued to the *ilia*." This evidently ſhewing that its principal organ is its lungs, and having no belly, ſhews it requires very little food; it having, in a manner, no organs to digeſt it.—"The thickneſs of its body is not to be determined, as the creature alters that at pleaſure whilſt it more or leſs inflates its body; and this inflation not only goes through the whole body, but into the legs and tail." How clearly nature's deſign is to throw into this animal as much air as poſſible, as it has peculiar organs for the purpoſe.

But I ſee, in the Monthly Magazine, that Mr. Gren has ſome experiments to prove that no pure air is received into the blood by reſpiration. But, nailed to this infatuated French theory, he ſuppoſes that the animal dies from ſuffocation, the lungs not diſcharging theſe phlogiſtic bodies* So this aceſcent vegetable food produces ſo much phlogiſton that the animal requires the lungs to be acting every moment, and, if they ſtop their function but for five minutes, death enſues. Alſo, it requires its animal heat of 96 to be conſtantly diſcharging its phlogiſton, as free fire; beſides, its diſcharges are ſo phlogiſtic. Nay, the Cameleon requires more than any other animal, all theſe diſcharges of phlogiſton which it received from the fly, O Wonderful!

I alſo ſee in the Monthly Magazine, an account of experiments that vegetables decompound pure air into fixed, and alſo decompound fixed air again; ſuch hocus pocus experiments and opinions are ſo groſsly abſurd that they deſerve no anſwer.

Men, under the influence of the French theory, are ſtill introducing moſt ſingular doctrines, miſconſtruing the moſt obvious experiments. See an account of M. De Saſſure, jun. experiments in the Monthly Magazine for the month of April, 1798. It ſays, "In the ſame valuable number we find an Eſſay by M. DE SASSURE, jun. on the queſtion, "Is the formation of carbonic acid eſſential to vegetation?" From ſeveral ingenious experiments on vegetation in atmoſpheric air, mixed with different proportions of carbonic acid, and in atmoſpheric air deprived of carbonic acid, Mr. De S. has deduced the following laws:

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* The abſurdities of which opinion ſee p. 72 of this Letter.

1. That plants, like animals, are continually forming carbonic acid while vegetating, either in the light or ſhade.

2. That like animals, they form this carbonic acid, by means of the oxygen of the atmoſphere; and that the reaſon why the formation of this acid is not always manifeſt, is its being immediately decompoſed.

3. That the preſence, or rather the elaboration of carbonic acid, is neceſſary to vegetation in the light.

4. That light is favourable to vegetation, by contributing to the decompoſition of carbonic acid.

5. That plants, while vegetating in the light, can ſupport a doſe of carbonic acid ſo ſtrong as to deſtroy them when in the ſhade."

The vegetable juices having a far leſs attraction for the fixed fire of the atmoſphere, therefore do not decompound its pure air into fixed air; but, only take a part of its fixed fire, bringing it nearer to the ſtate of azote. It is for this reaſon that vegetables require an airy ſituation, the vegetable receiving a part of the air's fixed fire, and freſh air is immediately applied by the wind, ſo that it may receive a part of its fixed fire; but not ſo much as to form the air into fixed air; which is eaſily aſcertained by receiving a current of air, which is conſtantly paſſing through a tube, wherein are placed a number of vegetables, and examining this air by lime water. But, if a vegetable is placed in cloſe veſſels, then it will not grow luxuriantly (not having the free current of the atmoſphere to give to it its ſuperabundant fire) and it will receive all the air's fixed fire, forming it to fixed air. Alſo, vegetables having a quantity of fixed air, mixed along with the atmoſpherical air, they are vegetating in, their watery juices will imbibe it, as water would do.* And even as I have all along ſuppoſed, this fixed air circulating in the vegetables, mixed with their juices, may receive ſuch a quantity of fire from the ſun as to be diſcharged as air; in the ſame manner as fixed air is formed into pure air, when united with water by the ſun's rays. And alſo, when it is floating in a moiſt atmoſphere, as I have ſhewn.—And that pump water, &c. which only contains fixed air, will by expoſure to the ſun, produce pure air, is well known: but, upon the water's generating the pure air, the water turns green, from a number of animalcules, as Dr. Ingenhouſz and others have ſhewn. And when the water is thus incorporated with theſe green animalcules, and having given out its pure air, if you go on impregnating it with fixed air, it will continue to generate pure air. Therefore when water, which before expoſure to the ſun, only gave out fixed air; after expoſure gives out pure air; and at the time when the pure air is diſcharged from the water, great numbers of green animalcules appear: allowed by every one. Then theſe green animalcules muſt act upon the fixed air by arreſting the ſun's rays, appears clear; for, ſilk does the ſame, otherwiſe, if theſe animals acted at all upon the air, it would be by decompounding pure air into fixed air, and not fixed air into pure air.

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* It is the ſame in the animal body; from great inanation the inhaling veſſels of the ſurface will imbibe, from the circumambient air, a conſiderable quantity of matter. See Dr. Watſon's Eſſays. A jockey, in the courſe of a ſhort time, how conſiderably heavier he got, though he had only drank a glaſs of wine. But, in a ſtate of health, or when the veſſels were properly filled, the ſyſtem would have diſcharged matter, inſtead of receiving it, Therefore, what vegetables do in this confined unvegitating ſtate, is no argument what they do in their healthy ſtate in the open air; they reſpire by their leaves, therefore, in great inanation, a ſimilar ſtate to the jockey, they will imbibe air and moiſture from the atmoſphere.

And, I ſhould hope, Mr. Cavendiſh, you will not aſſert that theſe animalcules differ ſo far from all other animals; that inſtead of diſcharging charcoal, as you ſuppoſe, by the air, they really imbibe it; for after the proceſs, no charcoal is generated.

That light, or rays of the ſun, are neceſſary to vegetation, for thoſe plants which do not receive them, are blanched, loſing that fine vegetable aromatic oil. Which is, from this cauſe, the rays of light receiving that impulſe or motion, penetrate the vegetable, and enter into its ſubſtance.—But heat, or fire, which has not that impulſe, does not enter ſo well into the vegetable, ſo as to unite with its ſubſtance; it is juſt the ſame with the metallic earths, or calces. By expoſing the calces of ſilver, mercury, gold, mangane ſe, &c. to the rays of the ſun (See Scheele, p. 77); the calces were reduced, forming metals; but, if they were expoſed to the ſame heat as thoſe rays produced, no reduction would take place; the fire not having that impulſe of motion as light, to penetrate the calces and enter into its ſubſtance, ſo as to reduce them. And, that it is heat alone which reduces metals, is certain; for, if you apply a greater degree of heat, which heat ſhall paſs through black bodies before it arrives at the calces; it will nevertheleſs reduce them.

Out of the numerous arguments I have brought in my different publications, to prove that alkaline ſalts are fixed fire, I ſhall quote one in my Chemical Eſſays. " In forming the calcareous earths into lime, in attending to the proceſs accurately, we may obſerve different phenomena; at firſt the fixed air and water are expelled, and the earth ſaturated with looſe fire. But if you puſh the proceſs farther, the fire will, inſtead of taking this looſe ſaturation, become more fixed, penetrating the earth, ſo as to become alkaleſcent: and in this ſtate the lime becomes light and ſpungy, having loſt part of its earth, and will not do for mortar, the workmen rejecting it, as being uſeleſs; for it will not ſplit and fall into powder with water. I have, by carrying on the proceſs for a long time, made it almoſt an alkaline ſalt, turning it perfectly mild, in reſpect to its cauſticity, and having the properties of alkalis in moſt of its qualities."†

Many of the leading philoſophers, after the publication of my Treatiſe on Air, ſeeing in what a ridiculous light I had placed their doctrine, and how untenable their own principles were; immediately fled to the French ſtandard, as the only one which could, in any manner, make any head againſt my ſyſtem; and hoping, by their numbers and great *names*, to ſuppreſs inveſtigation, reaſon, and truth.‡ But, *magna eſt veritas et prævalebit.*

Now

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† 'I have proved, in my Thoughts on Air, that the cauſticity of lime does not depend upon the abſence of its fixed air, but upon its ſaturation of looſe fire; which fire, when it becomes active by water, &c. burns or conſumes bodies. Now, this experiment proves it: for, when the fire is ſo puſhed in the proceſs, as to be more fixed in the earth, ſo that moiſture, &c. cannot expel it, the lime then becomes mild;—and it will bear no other explanation. The idea of its wanting its acid to neutralize the earth, is quite inadequate to the phenomena; for it ought in this caſe to have become more cauſtic at the latter part of the proceſs, inſtead of its becoming perfectly mild, and of an alkaleſcent quality.

‡ I ſee in a late publication, an opinion of mine given to Dr. Latham; which I had repeatedly publiſhed long before. That part of the electrical fire which is obſerved in a ſpark, comes

from

Now let us conſider what all their boaſted diſcoveries amount to; why, that the animal lungs are a coal pit. The air receiving by them, from the blood, the very identical ſame bodies which colliers draw from their coal pits, viz. coal and inflammable air. Let this *great diſcovery* be handed down to poſterity, as the important reſult of the experiments of our modern chemiſts.† Fame, let thy trumpet ſound to future ages this glorious *truth*, of the 18th century. Let poets paint ſome emblematical fiction, perſonating the philoſophers and the ſurpriſing coal pit.

Impreſt with this *great*, *wonderful*, and *ſurpriſing* diſcovery, my imagination painted to me the the ſcene in Macbeth; the witches, and Hecate with her "Vap'rous drop profound" caught from "the corner of the moon," dancing round the cauldron: emblematical of you, philoſophers, and the coal pit; with your profound magic art dancing around it, and with the light *airy* ſtep of a French cotillon: each philoſopher *throwing* into the cauldron ſome experiments and ſuggeſtions; and the

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from the air; the air being, in conſequence, injured. To prove which, I ſhewed the ſpark taken *in vacuo*, has not any luminous brilliancy. But theſe things are trifles, my name was never to be mentioned, for that was their great policy. Dr. Prieſtley, I have ſhewn, has taken many of his opinions from mine, but never once hints at my name.

† From calculating the quantity of pure air turned to fixed air, in the courſe of twenty-four hours, by one man; and the proportion, they ſay, of charcoal, which pure air requires to its forming fixed air; we are brought to this data, that many pounds of charcoal muſt be diſcharged by every man's lungs in the courſe of twenty-four hours: but, as coal conſiſts of both charcoal and inflammable air, they ſay, then it is nearer the body which the lungs diſcharge, ſo that many pounds of it muſt be paſſed through the tender veſſels of the lungs in twenty-four hours, without either injuring, or blackening them. O Wonderful proceſs, you wiſe aerial chemiſts, it is a moſt curious coal pit!

foremoſt of theſe *aerial* beings, which ſeemed loaded with the heavieſt burthen, and which even made the pot to boil over, was the two experiments of burning inflammable air and oxygen gas, forming water; and oxygen gas and azote forming the nitrous acid with the electrical fire. Upon theſe experiments being thrown in, the *air* rung with acclamation; even a number of ſnarling, barking, curs, added to the noiſe. At laſt a majeſtic figure appears, called truth; having in one hand reaſon, and in the other common ſenſe, from whom a little puff of wind aroſe and diſſipated the whole. They vaniſhed into airy nothing, "And left not a wreck behind."

Obſervations upon Count RUMFORD's *Paper.*

I ſhall now make a few remarks upon Count Rumford's Paper, in the Phil. Trans. He certainly deſerves every compliment from the public for his uſeful reſearches; but I muſt beg leave to make a few ſtrictures upon his paper, on firing gunpowder

Now, let us conſider what gunpowder is formed of: the ſtrongeſt, according to Dr. Watſon, is nitre eighty parts, charcoal fifteen, ſulphur five, of theſe ingredients nitre makes the largeſt proportion. We ſhall ſee, in the ſequel, that one pound of nitre is compoſed, according to Mr. Lavoiſier, of

Potaſh 7 oz. 6 gros. 51. 84 grs.=4515.84 grs.
Dry acid 8 — 1 —— 21.16 ——=4700.16 ——

The above quantity of dry acid is compoſed of

Oxygen 6 oz. 3 gros 66. 34 grs.=3738.34 grs.
Azote 1 — 5 —— 25. 82 ——= 961.82."—

The Count has clearly ſhewn that the force of firing gunpowder is conſiderably ſtronger than Mr. Robin's, who only made it one thouſand, times greater than the mean preſſure of the atmoſphere; while he makes it equal to fifty thouſand. But, the gunpowder, in the Count's experiment, had not all its force, as it was fired in a cloſe veſſel and had not liberty to expand in the atmoſphere; for, by this expanſion, all the nitre and charcoal are burnt; alſo the ſulphur is more thoroughly ignited, and their fixed fire ſet looſe. To prove this, if you fire gun-powder in pure air, its force will be conſiderably ſtronger, than when fired in atmoſpherical air. The experiment is to take a large decanter, with a bladder fixed to its mouth, and place in the bottom of it a piece of hot iron, and then, gradually, drop upon the iron, repeatedly, ſmall grains of the powder from the bladder. By this means you will be able to burn a great quantity of powder, and, you will find, according to the purity of the air, in the decanter, the powder will fire with different forces. By firing in this manner, very ſtrong powder, particularly that made with dephlogiſticated marine acid, and an alkali, in the pureſt air, all the fixed fire of the powder will be ſet looſe, viz. that from the alkali, which is the principal part; and alſo, that of the charcoal and ſulphur: ſo that there only will remain an acid vapour, with a little of the aſhes from the charcoal. When the reſiduum is to be examined accurately; either a piece of heated ſilver, or the electric ſpark, are the beſt to fire the gunpowder.

Now let us conſider the way in which Mr. Lavoiſier's theory explains it. *The ſuppoſed caloric*, in the nitrous acid, is attracted by the charcoal, forming fixed air. But, he himſelf is obliged to acknowledge. Mr. Lavoiſier ſays, p. 453. of his Nomenclature, " I have tried ſome " kinds which have produced almoſt double the " effect [meaning the force of the exploſion] of " ordinary gunpowder, although they give out a " ſixth part leſs of gas during deflagration."

As the ſulphur's attraction for this pure air could only ſet the ſuppoſed caloric looſe, for they generate no air, but condenſe air. Then of what ſervice is the alkali? none according to them; while every one who is acquainted with the compoſition of gun-powder, knows that all its force depends upon the quantity of the alkali it contains. Then, according to theſe wonderful theoriſts we ſhould find, after the operation, all the azote, or nitrous air, which the pure air of the acid was combined with; the alkali entire, united with the vitriolic acid, and an immenſe quantity of fixed air, which ſhould be formed from the pure air of the acid and the charcoal. They inform us that the great heat generated in forming phoſphorus, or ſulphur, into the phoſphoric or vitriolic acids, comes from the pure air being condenſed in the operation. Now, wonderful, that this already condenſed air, which they ſuppoſe, in this mineral acid, ſhould poſſeſs all this fire, ſo as to cauſe ſo wonderful an exploſion: nay, upon parting with its ſuppoſed azote or baſis, they ought to be aerilized. Can we believe a theory ſo prepoſterous and abſurd, which ſuppoſes that all theſe phlogiſtic bodies, which diſappear in the proceſs, do not contribute an atom of fire to the combuſtion; it all proceeding from the condenſed pure air in the nitrous acid. How hard it is, in this prejudiced trifling age, I ſhould have ſuch abſurdities ſo repeatedly to combat. But I ſhall conſider, more particularly, Count Rumford's experiments. He ſays, p. 233. " But what will become of this " theory, and of all the ſuppoſitions upon which " it is founded, if I ſhall be able to prove, as I " hope to do in the moſt ſatisfactory manner, that " the force of fired gunpowder, inſtead of being " 1000 times, is at leaſt 50,000 greater than the " mean preſſure of the atmoſphere ?"

Now he ſees the impropriety of attributing this immenſe force to the generating of airs, according to Mr. Lavoiſier's theory. But, which he ſeems to adopt; as the general principles of it are as neceſſary to eſtabliſh his own, which is that " To ariſe principally from the elaſticity of the *aqueous vapour* generated from the powder in its combuſtion." In order to find ſufficient water for this theory, he ſays, it is even generated in the proceſs. But they do not allow that any of theſe bodies contain inflammable air, to generate it: to carry their ſuppoſition ſo far, as, that the alkali contains it along with azote ; then we ſhould have this azote alſo ſet looſe, after the proceſs, ſo as to add to theſe wonderful generation of airs. But, Mr. Lavoiſier does not, *even hint* at ſuch a prepoſterous idea. The late experiments of Dr. Girtanner ſhew, that the alkali, according to their theory, is formed of pure carbone. Then the alkali would form with the pure air of the acid's ; fixed air, in the combuſtion. Good heavens! what a generation of airs there ought to be in this combuſtion. But two neceſſary queſtions intrude themſelves: from whence comes all the caloric to produce the immenſe fire in the combuſtion, and alſo to aerilize all theſe aerial bodies? and, if theſe airs are obſerved after the proceſs? The firſt queſtion they anſwer by ſaying, that all this wonderful caloric is *in the condenſed pure air*, in the nitrous acid. Aſtoniſhing! As to the other queſtion, they admit theſe airs are not generated. But the Count himſelf, with all his attachment to their theory, ſays, "it is dangerous "to admit the action of an agent (viz. caloric) "whoſe exiſtence is not yet clearly demonſtrated."

But what proves clearly that the charcoal is not eſſential in this fulminating proceſs, is, that nitre will fulminate with an alkali and ſulphur, without the charcoal; therefore, Mr. Lavoiſier's doctrine is directly contradicted: for here is no charcoal to unite with the ſuppoſed pure air of the acid. The true explanation is this, the nitrous acid being united with the alkali, along with charcoal and ſulphur; the charcoal as being a very combuſtible ſubſtance, being ground into a fine powder, and mixed with the other ingredients; upon fire being applied, it immediately takes fire; and as the ſulphur is the next body for being ſuſceptible of combuſtion, it alſo takes fire, and the heat generated is ſo conſiderable as to give that activity to the nitrous acid, that it penetrates the alkali, ſetting its fire looſe as actual. This acid, when applied to that phlogiſtic body, eſſential oil, will of itſelf, without the aid of heat, or pure air, ſet ſuch a quantity of their fire looſe as to generate combuſtion. If the alkali is united with the dephlogiſtic marine acid, the leaſt attraction, without heat, will make them explode, as we have juſt obſerved; or, if a powder be formed of nitre, an alkaline ſalt, and ſulphur, and heated to a certain degree, they will explode: a well known experiment. Here, in theſe proceſſes, they can only be explained upon my hypotheſis; the fire, ſet looſe from the alkali, unites with the nitrous, or marine acids, and flies of with them; cauſing the exploſion. Heat of itſelf has no exploſive force; it is owing to its being united to bodies as acids, water, &c. That it has any mechanical powers, is proved from this; form a hollow globe of gold, and heat it as intenſe as poſſible, the heat will not expand or burſt the globe. The Count ſays, that the exploſion is owing to water being formed into a caloric vapour. But it is ſurpriſing, that when an hypotheſis is formed, the moſt obvious and clear facts paſs unnoticed. The nature of this wonderful elaſtic vapour he has ſhewn us. He ſays, p. 226. "Upon examining the vent-plug and the "pin, they were both found to be much corroded "and damaged; though I had taken the precau"tion to harden them both before I made the ex"periment.

"I afterwards repeated the experiment with a "ſimple vent, made very narrow, and lined with "gold to prevent its being corroded by the acid "vapour generated in the combuſtion of the gun"powder; but this vent was found, upon trial, to "be as little able to withſtand the amazing force "of the inflamed gunpowder as the others. It "was ſo much, and ſo irregularly corroded, by the "exploſion in the firſt experiment, as to be ren"dered quite unſerviceable."

Let me aſk the Count this queſtion, will the vapour of water corrode gold? But here he himſelf allows it to be an acid vapour, and, will not this circumſtance alſo inform our obſtinate theoriſts, that the acid is not decompounded, but paſſes entire: and, I appeal to the Count's experiments if this clear elaſtic acid vapour is not the cauſe of the exploſion.* If the exploſion depended upon the decompoſition of the acid, it could only generate fixed air or water, according to their theory. But, if the Count will explode the fulminating powder, formed of nitre, ſulphur, and an alkali; the ſame acid vapour he will find eſcape, corroding the gold: or, if the metal be examined, after its corroſion, it will be found united to the nitrous acid. Theſe are plain, obvious and ſimple truths, which, it is impoſſible to miſtake; and, I call upon the Count either to accede, or invalidate this explanation of the phenomena. He alſo ſays, p. 248. "This ſubſtance, (meaning the reſiduum after the exploſion) "which was of a black colour, or rather of a dirty "grey, which changed to black upon being expoſed to the air, had a pungent, acrid, alkaline "taſte, and ſmelt like liver of ſulphur. It attrac"ted moiſture from the air with great avidity — "Being moiſtened with water, and ſpirit of nitre "being poured upon it, a ſtrong efferveſcence en"ſued, attended by a very offenſive and penetra"ting ſmell. Nearly the whole quantity of "matter of which the powder was compoſed, "ſeemed to have been transformed into this ſub"ſtance; for the quantity of elaſtic fluid which "eſcaped upon removing the weight, was very "inconſiderable; but this ſubſtance was *no longer* "*gunpowder;* it was not even inflammable."

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* If it was the water in the ſalts that made the exploſion, the moiſtening the powder would aſſiſt the exploſion; but every one knows it would counteract it, and, as to the French experiments of M. De Betancour, I have no great opinion of them.

Now here it clearly appears that the nitre had loſt its acid, as it efferveſced ſo ſtrongly with freſh ſpirit of nitre. But, according to the French theoriſts, the acid muſt have been decompounded, and its pure air generate with the carbone, fixed air; and alſo its baſe, azote, muſt have been ſet looſe: then what an immenſe volume of airs this fixed air and azote muſt have formed, being equal in weight to the acid and charcoal; according to this theory (which, the more it is inveſtigated the more abſurd it appears) the Count ſays, "The "quantity of elaſtic fluid, which eſcaped, upon "removing the weight, was very inconſiderable."

But let us ſee how far it agrees with my theory. Upon the exploſion, the fixed fire of the alkali, charcoal, and ſulphur eſcaped, and united itſelf with the acid of the nitre, and the ſulphur (for ſince the firſt days of chemiſtry, it has all along been taught that acids, particularly the nitrous, has the ſtrongeſt attraction for fire, either fixed or free) therefore part of the fire united with the acids, and which produced the exploſion; but as they were confined from eſcaping, they were forced, by the immenſe weight, to enter the maſs again.† And when freſh nitrous acid was added to the maſs, the old acids, united with the fire, eſcaped, producing "a very offenſive and penetrating ſmell."‡ I think I need not comment upon their theory, which ſays, all the fire comes from the condenſed pure air in the nitrous acid; for here is the pure air condenſed; and air, in its condenſation they ſay gives out all its fire or caloric, as in the burning of ſulphur and phoſphorus. Yet, in the nitrous acid, when this pure air eſcapes from its condenſation, that it ſhould give out ſuch an immenſe quantity of fire again, is truly laughable: nay, that it ſhall now aerilize, both its baſe, which they ſay is azote, and alſo itſelf, with the charcoal, forming fixed air. And this, they ſay, is proved from Dr. Black's doctrine of latent heat. Can we believe rational men could have produced ſuch a theory. *O you great aerial philoſophers!* You are true inhabitants of Swift's flying iſland. See my Letter in the Gentleman's Magazine, for Auguſt, 1796. I ſay in my Treatiſe on Air, p. 18 and 49.

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† As according to Dr. Darwin's experiments; upon air being ſtrongly mechanically compreſſed, it will give out heat: ſo of the nitrous acid in this proceſs, by the immenſe mechanical force exerted upon the nitrous acid vapour. part of its heat, or fire will be forced from its chemical combinatiou with the acid; and part of the acid will be forcibly condenſed into the alkali again; but, in a phlogiſticated ſtate, that is, ſtill retaining a quantity of fire.

‡ Mr. Scheele has obſerved, that when nitre has been much heated, even the vegetable acid can ſet its nitrous acid looſe.

'Now let us conſider more particularly the circumſtances, half an ounce of oxygen-gas, in being condenſed into the phoſphoric acid, is ſuppoſed (agreeably to their theory) in its condenſation, to produce heat equal to melt 34 oz. of ice: but, when a leſs quantity of oxygen-gas is already condenſed, and hath given out the heat, which it does in its condenſation, that it ſhall produce nearly an equal quantity of heat, ſo as to melt 32 oz. of ice, is an evident abſurdity. Beſides, as we have before obſerved, if this ſame quantity of nitre was applied to phoſphorus, it would have melted more than 34 oz. of ice. Oxygen gas, in its aerial ſtate, is not in the leaſt acid, while it is ſuppoſed to poſſeſs its caloric; but it loſes it upon being imbibed into the ſulphur, and phoſphorus forming the vitriolic and phoſphoric acids. Then, when it has entered into the nitrous acid, forming that ſtrong mineral acid, the firſt in point of acidity, at leaſt, next to the vitriolic acid, which, they ſuppoſe, contains no caloric.— Can we ſeriouſly think it takes with it all its caloric? Nay, it muſt poſitively take with it more than the oxygen gas poſſeſſes, as it produces in combuſtion with phlogiſtic bodies, more heat than could be produced from the ſame quantity of oxygen gas, which they ſay it poſſeſſes. But if we are to philoſophize upon this experiment, agreeably to the Drs. Black's and Crawford's theories, the wonder will be ſtill greater; for in the experiment of burning nitre and ſulphur, the ſulphur is formed into the vitriolic acid, and likewiſe different aerial bodies are formed; ſo much as, had the authors of this extraordinary theory, endeavoured to prove that a degree of heat, equal to the melting of 32 oz. of ice, had diſappeared inſtead of appeared, this experiment would have made them ride triumphant. But it is too abſurd for men who deſervedly rank ſo high in chemiſtry to defend. For heaven's ſake liſten to the voice of reaſon, nothing can be gained by ſuch a controverſy.*

'Chemiſtry has of late ſuffered very great innovations; and, if our modern philoſophical chemiſts go on as they have done, it is not known where they will ſtop. To me it has often been matter of ſurpriſe, that none of the old regular chemiſts have borne their teſtimony againſt ſuch degradations. They all ſeem to be in a lethargic ſtate, as if bitten by the *tarantula*.

'One of the firſt principles of chemiſtry is that of chemical attractions; and our anceſtors in this ſcience perſuaded, that theſe attractions are one of the moſt ſingular phenomena in nature, have handed down to us a regular table of them. Particular bodies have a very ſtrong attraction for each other, very different from the principles of gravitation. If two bodies, having an attraction for one another, are expoſed to each other's influence, in the medium of a fluid, they will coaleſce. Thus, if an acid and alkali are dropt into water, they will form a neutral ſalt: ſo alſo, whenever bodies are expoſed to each other in a fluid, or moiſt form, their attraction will take place. Fluidity ſeems to be neceſſary only to bring their particles into immediate contact.

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* It is wonderful how this French theory has been ſo generally adopted; and even, from their own confeſſion, how inadequate it is to account for the phenomena. Mr. Lavoiſier ſays, in his Elements, p. 106. "By the foregoing experiments, that "quantity of charcoal ſhould melt 76.18723 libs. of ice, and "the quantity of hydrogen in a pound of the oil ſhould melt "62.15053 libs. The ſum of theſe two gives 138.33776 libs. "of ice, which the two conſtituent elements of the oil would "have melted, had they ſeparately ſuffered combuſtion, whereas "the oil really melted 148.88330 libs. which gives an exceſs of "10.54554 in the reſult of the experiment above the calculated "reſult, from data furniſhed by former experiments." The reaſon is this, the oil contains ſpecifically more fire than charcoal.

'But how are our preſent chemical attractions conducted by combuſtion? Combuſtion, inſtead of allowing bodies to unite, acts as a ſtrong mechanical power in breaking them down. On the contrary, according to their chemical attractions, combuſtion, (which is the ſetting looſe an eſſential part of moſt chemical bodies, viz. phlogiſton or concentrated fire, let looſe as actual fire) unites, but does not ſeparate them.

'Let us here give an example. I will take a ſalt, as it is a body (the chemical principles of which we have been fully inſtructed in by our forefathers) and alſo an acid, with which we are well acquainted: though this knowledge has been much obſcured by ſome late chemiſts. If the volatile alkali is aerilized into an air or vapour, and after that expoſed to the vapour of the nitrous or marine acid, they will unite, ſo as to form the nitrous or marine ammoniac ſalt; and the heat which kept them in an aerial ſtate becomes ſenſible. But if this alkaline air is expoſed to ſtill greater heat, it will then become inflammable air; and if the ſame nitrous vapour is added to this air, as in the former experiment; and then fired, it will emit an immenſe quantity of heat and light;

even

even fifty times more conſiderable, than came from it in the former experiment; upon examining the reſiduum, an acid water is found. Here the volatile ſalt has diſappeared, and fire appeared, or was diſcharged in the operation. Indeed, in the former experiment no light comes from it, but only a ſmall quantity of heat. Dr. Prieſtley ſays, vol. iii. p. 415. "Very little heat is produced by the union of acid, or alkaline air and water, though, as I have found by experiment, there is *ſome* produced, whereas the decompoſition of dephlogiſticated and inflammable air never fails to produce a very great degree of heat."

'Then thoſe who may be called our fathers in chemiſtry were convinced that the acid and volatile ſalt, by being united by a chemical attraction, would form a neutral ſalt. This now is perfectly abſurd; the true attraction does not take place till the combuſtion. And the true union of the alkali and acid is not a neutral ſalt, but water and acid. Such abſurd doctrines need no comment.

'The very ſame farce has been acted in the experiment of firing the oxygen and inflammable gas. Water and an acid are in the reſiduum.—The inflammable air, and alſo the fire, which neutralized the acid of the oxygen gas, have diſappeared, as has been long ago fully proved by Dr. Harrington. The diſappearing of the inflammable and dephlogiſticated air, an acid and water being left in the reſiduum, is (as Drs. Prieſtley and Harrington found) the very ſame reſiduum as that which is left, when the nitrous vapour and inflammable alkaline air are fired. In one caſe our aerial philoſophers cannot deny but that the acid which is left, came from the nitrous vapour and the water; heat and light, from the inflammable alkaline air. Therefore, by the ſame argument, we muſt equally allow, that the acid, in the other experiment, came from the dephlogiſticated air, and part of the water from the inflammable air. Nothing can be more plain and obvious. Yet our modern chemiſts will not allow theſe firſt and moſt common principles to be juſt ones; namely, that alkalis attract acids and neutralize them, and that phlogiſton neutralizes acids.*

If the neutral ſalt, which is formed by the marine dephlogiſticated acid, and the fixed alkali are ſet on fire; as neither of theſe bodies are aerilized in the proceſs, ſo there is leſs myſtery in their operation. Before combuſtion they formed a neutral ſalt; but after the combuſtion the alkali has diſappeared. What has become of it? We are altogether at a loſs to anſwer this queſtion according to their theory, unleſs they could introduce the art of *legerdemain* into their experiments of glaſſes, gun-barrels, &c. But here the aid of conjuration is ſtill farther required; for, as one *demon* diſappears, another, in the ſhape of an immenſe quantity of light and heat makes his appearance. However, as the tricks of the juggler ceaſe to ſurpriſe us when his art is known, ſo, had we not been ſo eaſy of belief and ſo fond of the marvellous, we would have paid a little more reſpect to what our chemical fathers have taught us; nay, I had almoſt ſaid what we may be convinced of by our own ſenſes. It is hardly poſſible not to give our aſſent to this truth, namely, that combuſtible bodies, or phlogiſton when fired, are changed into actual heat. By a proper attention to chemical principles, we ſhould have believed, that phlogiſton is concentrated or fixed fire. Yet it often happens, that as new diſcoveries pleaſe, merely becauſe they are new, and every one is anxious to aſcribe all phenomena to them, ſo, upon the diſcovery of Dr. Black's elegant doctrine of latent heat (which becomes latent in fluids, vapour, &c.) our modern chemiſts greedily graſped at it, and forced it, head and ſhoulders, into their preſent aerial fabric. But Dr. Harrington hath ſatisfactorily ſhewn, that the heat in theſe phenomena is chemically united, the ſame as in alkalis and acids.'

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* "Dr. Crawford in his explanation of the nitrous acid deflagrating with combuſtible bodies, ſays, that the fire firſt ſet looſe in the firing them, is the cauſe of this. And agreeably to him, this fire runs the gauntlet, of alternately aerilizing the oxygen gas of the acid, and of being conſumed. To ſuch ridiculous explanations are they obliged to have recourſe in ſupport of a wrong theory. I think ſuch powerful phenomena are not to be accounted for by ſuch ſlight cauſes."

I have now taken a review of all the chemical papers in the Trans. except Mr. Tennant's, upon the combuſtion of the diamond. And here, Mr. Cavendiſh, you muſt excuſe my not entering into any diſcuſſion of it. For, to endeavour to treat with ſerious argument, that abſurdity of abſurdities, viz. that the diamond is pure charcoal, would be an equal abſurdity, though it has had the ſanction of the Phil. Trans. I have related a ſimilar abſurdity which this theory of yours has given birth to. See page 75.

The beſt of all manufactories would be burning charcoal into diamonds which is only chryſtallizing it; for, Mr. Cavendiſh, your chemical powers certainly can do it. The great Newton ſuppoſed that the diamond is a combuſtible body, from its refraction of the rays of light; and that the refraction of light is from bodies poſſeſſing fixed fire, is clearly proved, from the repulſive principle of fire; a principle ſo certain, that it alone more clearly proves the diamond's compoſition,

poſition, and is an evidence ſuperior to all the vague rapſodies and romances of the French theory. That all bodies which contain fixed fire ſhould be formed of either carbone or inflammable air, is truly ridiculous; but, that according to the quantity of fixed fire bodies poſſeſs, they will, accordingly act upon the air. If not highly concentrated, they will leave the acid of the air in the ſtate of fixed air, or its aerial ſtate; as, in the burning of the fixed alkalies. See experiments of Dr. Girtanner. But, the more highly concentrated, as is the volatile alkali, its fire will be ſo intenſe as to decompound the air into a condenſed acid and water; as in the firing of inflammable air, which the volatile alkali may be formed into.'

The philoſophers of the day adopt the new theory in explaining all the phenomena, forcing it in head and ſhoulders, without queſtioning its validity; but even, if true, how could it rationally account for this particular phenomenon? viz. Count Rumford, in his Eſſays, when ſpeaking of food ſays, "In heating vegetables in water, the water adds to the nouriſhment." Then it muſt be by being decompounded by the proceſs of heating. If it was decompounded by *this ſimple proceſs* how is it performed? Is the inflammable air attracted by the vegetable in this *low temperature of heat*, below boiling? Then what becomes of the pure air? Or if the pure air is attracted what becomes of the inflammable air? The body which attracts the pure air is charcoal, then they ought to have formed fixed air; as to the inflammable air there was nothing to attract it but pure air which it was already united to. But our philoſophers have decreed it to be ſo, therefore, "*it muſt and does.*" The advantage reſulting from the proceſs is this; the vegetables and water by the action of the fire, are ſo united, or digeſted, that all their parts are more aſſimilated to enter into chyle; alſo another eſſential circumſtance is, theſe vegetable bodies, receive, in the proceſs, ſuch a quantity of the fire as animalizes them the more. Thus barley, from the proceſs of malting and brewing, receives ſuch a quantity of fire from the air in malting, and from the fire, in brewing, as conſiderably to alter its nature and qualities. But, in the Count's calculations he forgets how much more eaſily a Bavarian ſoldier, from habit and from the climate, is ſupported with food, than an Engliſh ſoldier.*

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* But pray, Mr. Cavendiſh, explain this experiment of Dr. Prieſtley's, in Vol. 4th, p. 36; alſo the note in page 33 of this Letter, upon the phoſphoric acid and the calx of lead producing inflammable air; but they ought to have produced dephlogiſticated air according to your theory.

In this Letter I have juſt examined your theory, principally in one department of nature, viz. reſpiration; but I am able to prove the ſame abſurdities in any of the reſt.

NOW, SIR, BEFORE I CONCLUDE, LET ME CALL UPON YOU IN THE MOST SOLEMN, AND SERIOUS MANNER, EITHER PUBLICLY TO REFUTE, OR ACKNOWLEDGE MY SYSTEM. BUT, IF YOU STILL CONTINUE *mute*, I THINK YOUR MOST PREJUDICED FRIENDS CAN BE AT NO LOSS FOR THE CAUSE.

I AM

Your moſt obedient and humble Servant,

ROBERT HARRINGTON.

Carliſle, JUNE 10, 1798.

FINIS.